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PSYCHOLOGY AND PHILOSOPHY

I.—PROFESSOR MARC-WOGAU'S *THEORIE DER SINNESDATA*[1] (I).

BY C. D. BROAD.

INTRODUCTION.

PROFESSOR Marc-Wogau's *Theorie der Sinnesdata* is an exceptionally comprehensive book. Four closely connected strands can be distinguished in it. (1) A very full, fair, and clear synopsis of nearly all the most important books and papers by writers who have either used, or considered and explicitly rejected, the 'sense-datum' terminology in discussing the philosophy of sense-perception. (2) A positive theory on this subject, which differs from that held by any of these philosophers. (3) An elaborate discussion of certain general philosophical concepts, such as the notions of *Thing*, *Existence*, *Individual*, etc. Marc-Wogau holds that such a discussion is needed, both as the basis for his own theory, and because the writers with whom he is concerned have used these notions uncritically and without drawing the necessary distinctions. (4) Criticisms, both in principle and in detail, on the arguments used and the conclusions reached by the authors whose theories are summarised. These criticisms are based partly on the general results reached in what I have called Strand 3, and are partly independent of these. Their primary object is to show that conclusions at variance with Marc-Wogau's own theory are at any rate unproven. But many of them are of independent interest.

[1] *Die Theorie der Sinnesdata*, Konrad Marc-Wogau, Uppsala: A.-B. Lundequistska Bokhandeln, pp. vi, 449.

In discussing the book I shall ignore Strand 1 almost entirely; for I find very little to criticize in Marc-Wogau's accounts of other men's theories, and there is no point in summarizing summaries. I shall concentrate my attention mainly on Strands 2 and 3. In connexion with Strand 4 I shall consider only those criticisms of principle which are an important part of Marc-Wogau's defence of his own theory, and some few of the detailed criticisms which happen to have interested me specially.

(A) *Preliminary Remarks on Terminology.* Before going further I wish to state and exemplify the terminology which I am going to use in discussing the problems raised by Marc-Wogau. It is obviously very important to have a terminology which does not beg any of the numerous questions at issue in his book. It may be remarked that Marc-Wogau confines himself almost entirely to cases of *visual* perception. I shall do this also, though I think that the tendency among philosophers to make this restriction has had a most unfortunate effect on their treatment of the problems of sense-perception. In any case the terminology which I shall use could very easily be adapted to tactual and to auditory perception.

The most general term which I shall use is *Visual Experience.* A feature common and peculiar to all such experiences is that they are experiences of *colour,* if we include under that heading black, white, and grey as well as the various hues.

Now visual experiences may be sub-divided as follows. There are some which the person who is having them would naturally describe at the time by saying 'I am seeing so-and-so' or 'I seem to see so-and-so', or which he would describe in retrospect by saying 'I saw so-and-so' or 'I seemed to see so-and-so'. We will call these *Ostensible Visual Perceptions.* There are others which the experient would naturally describe by saying 'I had such and such a mental image' or 'I saw so-and-so in my mind's eye'. We will call these *Visual Imagings.* We shall be concerned only with Ostensible Visual Perceptions.

An ostensible visual perception can be considered from two points of view. (i) We can ask: 'Is there in fact an object answering to the description which the experient gives of the object which he claims to be seeing, and is he in fact perceiving such an object?' This question can be put in the form: 'Is the ostensible visual perception *veridical* or *non-veridical?*' (ii) We can ask: 'Is the ostensible visual perception evoked by light coming to the percipient's eyes from an external object?' This question can be put in the form: 'Is the ostensible visual perception *normally evoked* or *abnormally evoked?*'

The answer to the second question often has a very close connexion with the answer to the first. An ostensible visual perception which is abnormally evoked is generally wholly or largely non-veridical. An ordinary dream, or the visual experiences of a delirious waking person, are examples of ostensible visual perceptions which are both abnormally evoked and non-veridical. But the questions are different, and it is important to distinguish them for the following reasons. (i) There are plenty of ostensible visual perceptions which are normally evoked but are non-veridical. Examples would be the experience called 'seeing a mirage' and in general any kind of optical illusion. (ii) In the records of psychical research there is extremely good evidence for the occurrence of ostensible visual perceptions which are abnormally evoked but are veridical. Examples are certain dreams and waking hallucinations which agree with certain contemporary but remote scenes and events to a degree which it would be fantastic to ascribe to chance-coincidence. Even if all such evidence were rejected, it would plainly be undesirable to adopt a terminology which ruled out the possibility of such cases by definition. So I shall sub-divide ostensible visual perceptions into *veridical* and *non-veridical*, and I shall subdivide each of these classes into *normally evoked* amd *abnormally evoked*.

I think that the word 'see' is commonly used in such a way that it would be incorrect to say that a person was 'seeing' so-and-so unless one believed that he was having an ostensible visual perception which was both veridical and normally evoked. Suppose that one was describing what one knew to be a dream or a waking hallucination. Then, even if one knew that it corresponded to some distant contemporary scene to a degree beyond all question of chance-coincidence, one would say 'He (or I) *seemed* to see so-and-so', and not 'He (or I) *saw* so-and-so'. Suppose that one was describing what one knew to be a misperception. Then, even if one knew that it was normally evoked, one would use some such phrase as, *e.g.*, 'I *seemed to see* a man pointing a gun at me, but what I *really saw* was a scarecrow in the twilight'.

Now, whatever may be the case with certain other kinds of sensible experience, *e.g.*, those of taste or of smell, the following seems to be true of all or nearly all visual experiences, and certainly of all those which could be described as ostensible visual perceptions. It is natural to describe such experiences by using a transitive verb, *viz.*, 'see' or 'seem to see', followed by a grammatical object which consists of a general name or an explicit description, *e.g.*, 'a penny', 'a flash of lightning', 'a

brown flat disk', 'a blue cross on a red field', and so on. I shall express the fact which I take to lie behind this linguistic usage by saying that in any ostensible visual perception there can be distinguished two factors, *viz.*, an *act of awareness* and an *object of awareness*.

Now suppose that a person is describing an ostensible visual perception which he is having. He will very often use the expression 'I see a so-and-so', where 'so-and-so' is either a general name for a class of material things, *e.g.*, 'penny', or is an explicit description of a material thing, *e.g.*, 'flat round disc made of copper and used as a coin'. If he does this, he is using a description which plainly asserts or implies a great deal more than is guaranteed by his present visual experience alone. *E.g.*, to call an object a 'penny' implies that it is something that could be felt as well as seen, something which other people could see and touch, something made of copper and issued by the British mint as a coin, and so on. The expression 'I see a penny' might be used equally by a person in any of the following situations. (i) A person who was looking straight down on a penny lying on his table before him. (ii) A person who was looking so obliquely at a penny lying on his table that it looked elliptical and not round. (iii) A person under hypnotic suggestion who had a visual hallucination of seeing a penny on his table, though there was in fact no such object there at the time. In the second of these examples he says, and says correctly and truly, that he is seeing a penny, *i.e.*, a *round* object. But, if he were to confine himself to describing the object of his awareness as it here and now *looks* to him, he would have to say that he is aware of an object that *looks elliptical*. In the third of these examples he is altogether mistaken in saying that he sees a penny, though he may be using the word 'see' correctly relatively to his beliefs at the time; for his ostensible perception is neither veridical nor normally evoked. He would admit this when he recovered his normal state and it was explained to him that he had been hypnotized. But, if he had confined himself to saying: 'I have a visual awareness of an object that looks brown, and flat and round and, in short, penny-like', there is no reason to suppose that he would have been mistaken.

For these reasons it is essential to distinguish conceptually the following two notions, *viz.*, (i) the object which is *visually prehended*, and (ii) the object which is *ostensibly seen*, in any ostensible visual perception. The distinction may be drawn as follows. Suppose that the experient, in describing such an experience, were to confine himself to describing how its object here and now

looks to him. Then I should call the object thus described a *visual prehensum ;* and I should say that it is *visually prehended as having* those characteristics and only those which the experient would ascribe to it if he confined himself entirely to describing how it here and now looks to him. Suppose, on the other hand, that the experient, in describing his ostensible visual perception, does not thus limit himself. He describes it by reference to an object which he asserts or implies to have characteristics other than those which are here and now visually presented to him. He does this, no doubt, *on the basis of* his present visual prehension, but he goes beyond (and perhaps even against) what he here and now visually prehends. Then I should call the actual or possible object thus described an *ostensible visum.*

The examples given above illustrate the following facts. (i) There may be nothing at all corresponding to the ostensible visum of an ostensible visual perception, *i.e.*, nothing actual which remotely answers to the description of that possible object. In that case the visual experience may be described as *wholly delusive.* But there will still be a certain visual prehensum, and there may be no reason whatever to doubt that it has the properties which the experient would ascribe to it. (ii) The description of the ostensible visum will always include or imply properties which are not visually prehended in the prehensum. Some of these may be properties which could not be *visually* prehended, *e.g.*, coldness and smoothness. Some of them may be properties which could not be *sensibly presented* at all, because they involve what I call ' categorial characteristics ' such as cause and substance. Examples would be causal and dispositional properties, such as impenetrability, mass, etc. (iii) The description of the ostensible visum may include or imply a certain determinate characteristic which could be visually prehended but which differs specifically from one which the prehensum is prehended as having. *E.g.*, when the penny is viewed very obliquely the prehensum is prehended as *elliptical,* but the description of the ostensible visum as a " penny " implies that the latter is *round.* But in this case there is something answering to the description of the ostensible visum. The ostensible visual perception here may be said to be *wholly veridical.* For the ellipticity which the prehensum is prehended as having does not lead the percipient to ascribe ellipticity to the visum ; on the contrary, in conjunction with certain other features in the total prehensum, it leads him to ascribe roundness to the visum. (iv) There are plenty of intermediate cases where there is good reason to believe that an ostensible visual perception is neither wholly delusive nor wholly veridical. There is

something actual which answers in the main to the description of the ostensible visum, and which the percipient is perceiving; but it differs in certain details from the latter. In such cases we may say that the visual experience is *veridical in principle but delusive in certain details.*

When an ostensible visual perception is not wholly delusive we can talk of its *actual visum*, and we can compare this with the description of that possible object which is its ostensible visum.

I can hardly hope that this terminology is impeccable, but I hope that it may serve. It is intended to leave open for discussion the following questions. (i) Suppose that there is an actual visum, answering completely or in principle to the description of the ostensible visum of a certain visual experience. What is the relation of the visual prehensum to the actual visum? Are they necessarily *different* entities, inter-related in some specially intimate way? Or is it possible that the visual prehensum and the actual visum may be *one and the same entity* under different descriptions, as, *e.g.*, the Prime Minister of England and the Leader of the House of Commons might be? (2) If this is possible, is there any good reason for or against holding that it is true? (3) Is it possible for a visual prehensum to have characteristics *besides* those which it is visually prehended as having, *e.g.*, to be in fact cold and smooth as well as brown and elliptical? (4) Is it possible for a visual prehensum to have characteristics *incompatible with* some of those which it is visually prehended as having, *e.g.*, to be in fact round though it is visually prehended as elliptical? It is plain that the answers to Questions 3 and 4 may have an important bearing on the answers to Questions 1 and 2. These are the kind of questions which Marc-Wogau is mainly concerned to discuss, and I am naturally anxious to use a terminology which shall not beg any of them.

(B) *Preliminary Sketch of Marc-Wogau's Theory.* We can now give a very rough preliminary sketch of Marc-Wogau's main contentions in Strands 2 and 3. I think that the following are the most important points.

(1) He thinks that most writers who would commonly be described as supporters of some form of the Sense-datum Theory have held that the visual prehensum and the actual visum are *different* entities even when the visual perception is wholly veridical. They have held various views about the nature of the actual visum, and correspondingly various views about the nature of the relation between the visual prehensum and the actual visum. But they have never admitted that the visual prehensum and the actual visum could be one and the same entity under

different descriptions; and they have produced many arguments to show either that this is impossible or that it is never in fact true.

(2) Most of them have either explicitly asserted or tacitly assumed that a visual prehensum can have no characteristics *besides* those which it is visually prehended as having, and *a fortiori* that it can have no determinate characteristic *incompatible with* any which it is prehended as having. If either of these contentions is admitted, Question 1 above must be answered in the following way. A penny, *e.g.*, is certainly a round hard thing made of copper; and what I visually prehend when I look at a penny is never prehended as hard or made of copper, whilst it is prehended as *elliptical* and not as round if I am looking very obliquely at the penny. Therefore, unless what I visually prehend can have certain characteristics *besides* those which I prehend it as having, and certain determinate characteristics *incompatible with* some of those which I prehend it as having, it cannot be identical with the penny which is the actual visum.

(3) Marc-Wogau holds that in certain cases, *e.g.*, when a penny is looked at directly and not very obliquely from about arm's length in normal light by a person in the mental attitude of ordinary practical life, there is no reason to doubt that what is visually prehended is *in a certain sense* identical with the actual visum, *i.e.*, with the penny itself. In other cases, *e.g.*, when the penny is looked at in a very oblique direction or from a very great distance or when the observer is in certain other mental attitudes, the visual prehension is certainly not identical *in that sense* with the actual visum. It is then an entity which stands in a certain peculiar relation to a certain other qualitatively different entity, *viz.*, the penny itself. In such cases, and *a fortiori* in wilder cases, such as vision in a mirror, waking hallucination, dreaming, etc., it is proper to speak of the visual prehensum as a 'sense-datum'.

It will now be clear why Marc-Wogau discusses certain very general problems which are not usually treated explicitly by writers on the philosophy of sense-perception. The fundamental question at issue is whether the prehensum and the actual visum of a visual perception are ever *identical* or must always be *different*. Therefore the various meanings of 'identity' and 'difference' need to be discussed. Again, the actual visum is in general a *material thing*, *e.g.*, a penny. Therefore it is necessary to discuss the notion of 'material thing'. In particular it is desirable to consider the following two notions, *viz.*, (i) the identity of a thing through time and change, and (ii) the relations between one thing and its many appearances at a given moment.

XUM

Now among the reasons which have been given for holding that the visual prehensum and the actual visum are always different the following have been prominent. (i) It has been alleged that the prehensum of any visual experience certainly *exists*, whilst it is always possible to doubt whether anything exists answering to the description of the ostensible visum. (ii) It has been alleged that the visual prehensum is always a *particular individual*, whilst a material thing is of a different logical type, *e.g.*, a *class*. It is therefore desirable to clear up the notions of 'existence' and of 'particular individual' and to point out any ambiguities which may lurk in them.

Marc-Wogau's discussions of these several very general problems occur at various parts of his book, interspersed between the treatments of much more detailed problems specially concerned with sense-perception. It seems to me to be convenient, now that I have explained why Marc-Wogau thinks it necessary to deal with these general problems, to take them all together and to clear them out of the way at the beginning. For Marc-Wogau's arguments and conclusions about specific problems in the philosophy of sense-perception depend in part on his answers to these general questions.

SECTION I. GENERAL PHILOSOPHICAL PROBLEMS

Under this heading I shall treat in turn Marc-Wogau's discussion of the following topics. (A) Particular Individuals, (B) Identity and Difference, (C) Existence, and (D) Material Things. (The order in which I take them is not in all cases the same as that in which they occur in Marc-Wogau's book.)

(A) *Particular Individuals.* Marc-Wogau's discussion of this topic is to be found in Chapter III, which is entitled *On Individual and Universal Objects*. I must confess that I do not find it easy to follow, and it is quite likely that I have only imperfectly understood it.

One thing at least is certain. Marc-Wogau holds that the notion of a particular individual presupposes the notion of what he calls a '*Bestimmungs komplex*'. This has to be distinguished from what he calls a '*Bestimmung*', whether compound or non-compound. We must now try to grasp these notions by means of Marc-Wogau's statements and illustrations.

We are told, on the top of page 156, that the word '*Bestimmung*' is used in the book in a wider and a narrower sense. In its wider sense it means 'anything that can be predicated in any sense of any object'; so it can presumably be translated by

'predicate'. When it is used in this sense Marc-Wogau says that 'certain *Bestimmungskomplexe* are also *Bestimmungen*'. So it appears that some (though presumably not all) *Bestimmungskomplexe* are predicates. But in the present context the word '*Bestimmung*' is used in a narrower sense. As regards this we are told (i) that no *Bestimmungskomplex* is a *Bestimmung* in this sense, and (ii) that every *Bestimmung* in this sense is a 'determinable or determinate'. It is not asserted that every determinable or determinate is a *Bestimmung* in this sense; but this is not denied, and I suspect that it is intended.

Examples of *Bestimmungen* in the narrower sense are 'red', 'triangular', and 'equilaterally triangular'. It seems to me that what is common and peculiar to all of them is that each is a certain relatively determinate form of a certain relatively indeterminate predicate. Thus, to be red is to be coloured in a certain way; to be triangular is to be shaped in a certain way; and to be equilaterally triangular is to be triangular in a certain way. If so, we may perhaps translate '*Bestimmung*', in the narrower sense, by the phrase 'specified generic predicate'.

Marc-Wogau points out that there are two different kinds of specified generic predicate. 'Red' is an example of the one, and both 'triangular' and 'equilaterally triangular' are examples of the other. The difference may be put as follows. 'To be red' is simply to be coloured *redly*. One cannot express the differentia between the specific predicate 'red' and the generic predicate 'coloured' except by repeating the word 'red' (or some linguistic equivalent of it) in an adverbial form. But 'to be triangular' is to be a rectilinear plane figure with three sides, and 'to be equilaterally triangular' is to be triangular with equal sides. Here the differentia can be analysed and expressed in independent terms. We could make a person understand what it is to be triangular, even though he had never seen an instance of triangularity, provided that he understood what it is to be a rectilinear plane figure and was acquainted with the notion of cardinal numbers in general and the number three in particular. Marc-Wogau calls specified generic predicates of the former kind 'non-compound' (*einfach*), and those of the latter kind 'compound' (*zusammengesetzt*). He points out that a predicate which is non-compound in this technical sense may yet be 'complex' (*komplex*) in another sense. Anything that is red, *e.g.*, has redness of a certain shade, a certain intensity, and a certain saturation. I propose to call such factors as these 'dimensions of comparability'. (In the case of sound, *e.g.*, the dimensions of comparability are pitch, loudness, and tone-quality.) Thus we may

state Marc-Wogau's doctrine here by saying that a specified generic predicate which is non-compound, in the sense that its differentia cannot be analysed and expressed in independent terms, may yet be complex, in the sense that it has several dimensions of comparability.

We come now to *Bestimmungskomplexe*. The predicate of 'being a red triangle' is given as an example. If we compare this with the predicate 'equilaterally triangular' or 'intensely red', we see that the difference may be put as follows. To be equilaterally triangular is to be triangular in a *certain way*; and the same is true *mutatis mutandis* of being intensely red. But it would be nonsensical to describe an object which was red and triangular as either 'redly triangular' or 'triangularly red'. It is redly *coloured* and triangularly *shaped*. The connexion between the two relatively indeterminate predicates of 'being coloured' and 'being shaped', and the consequent connexion between the relatively determinate form of the one and the relatively determinate form of the other (*e.g.*, between being *redly* coloured and being *triangularly* shaped) which happen to belong to the same object, is something unique and peculiar. It is obviously completely different from the connexion between a relatively indeterminate predicate and the relatively determinate form of it which happens to belong to a certain object, *e.g.*, between the predicate of being triangular and that of being equilaterally triangular in the case of an object which happens to be an equilateral triangle. I propose to call it 'compresence'. It must be understood that this phrase is not intended to imply or to exclude any particular theory about substance and attributes.

I have stated all this in my own way, but I think that it agrees with what Marc-Wogau has in mind. I will now add the following remarks. In the case of a *Bestimmungskomplex* we are concerned with what might be called 'relatively contingent compresence'. It is, perhaps, necessary that anything which has colour should have shape, and that anything which has shape should have colour. On that question I do not wish to express a decided opinion here. But, if a certain thing is, *e.g.*, red and triangular, its redness and its triangularity are contingent to each other. It might have been red and had any other shape, and it might have been triangular and had any other colour.

Marc-Wogau points out that such a complex predicate as 'red-and-hard-and-circular' is the most elementary kind of *Bestimmungskomplex*. Consider the case of an object which might be described as a circular area with one half red and the other half green. The predicate here is also an example of a

Bestimmungskomplex. Plainly the predicates 'red' and 'green' are contained in it in a different way from that in which the predicates 'red', 'hard' and 'circular' are contained in the *Bestimmungskomplex* 'red-and-hard-and-circular'. So we must recognise that a *Bestimmungskomplex* may have a very elaborate internal structure.

Marc-Wogau says that certain predicates can, and others cannot, be combined into a *Bestimmungskomplex*. He says that this is an ultimate fact. The examples which he gives to illustrate this alleged fact are the following. 'Red' and 'triangle' can be so combined; 'red' and 'hard' cannot; but 'red' and 'hard' and 'surface' can.

I suppose that the ground for his statements is the following. It is grammatically correct to describe an object as 'a red triangle' or as 'a red hard surface'; but it is not grammatically correct to talk of a 'red hard' or a 'hard red'. It seems to me that there is nothing substantial in this distinction, and that it depends simply on a contingent peculiarity of the English and the German languages. We happen to have a substantive-word 'triangle' for surfaces whose shape is triangular, but no substantive-word for surfaces whose texture is hard or surfaces whose colour is red. Suppose that we could talk of 'a red' or 'a hard', as we can talk of 'a triangle'. Then we could talk of 'a triangular red' as well as of 'a red triangle'. And we could talk of 'a red hard' or 'a hard red'. In all these cases the predicate can be expressed by such phrases as 'red-and-triangular' or 'red-and-hard'; and then all appearance of the alleged difference vanishes. In *both* cases it is presupposed that the object is, or has, a surface; for each of the predicates 'red' and 'triangular' and 'hard' is a specific modification of a generic characteristic which can belong only to extended objects.

We come next to the case of common nouns, *e.g.*, 'book' or 'penny', used as predicates, *e.g.*, in such phrases as 'That is a book' or 'There is a penny on the table'. Marc-Wogau holds that these express a certain special sub-class of *Bestimmungskomplexe*. I take it that what he means is this. By describing an object as 'a book' or 'a penny' we are ascribing to it a complex predicate composed of relatively contingent compresent simpler predicates; although these simpler predicates are not explicitly named, as when we describe an object as a 'red hard triangle'. A common noun, used predicatively, may be said to express an *implicit Bestimmungskomplex*.

Finally we come to the objects denoted by such phrases as 'This *X*', where '*X*' is either an explicit *Bestimmungskomplex*-phrase,

e.g., 'red hard triangle', or an implicit *Bestimmungskomplex*-phrase, *e.g.*, 'book' or 'penny'. According to Marc-Wogau, any such phrase denotes a *Bestimmungskomplex* of the following peculiar kind. Among its constituents are included a *local and a temporal* predicate, and these are *completely determinate*. If I understand him aright, the phrase 'This red hard triangle', *e.g.*, denotes what might be otherwise expressed by the phrase 'Redness, hardness, and triangularity compresent here now'. And I suppose that the phrase 'this penny' would denote what might be otherwise expressed by the phrase 'Such and such predicates (*viz.*, those which constitute a definition or description of a penny) compresent here now'.

There are two explanatory comments to be noted here. (i) It is admitted that one might look at a certain object and say of it correctly and truly: 'That table was in my grandfather's house. I inherited it. It has been away to be polished and has lately come back'. For an account of Marc-Wogau's complete treatment of this the reader must wait until he comes to Sub-Section D of the present Section of this paper, where I deal with the concept of Material Thing. It must suffice for the present to say that, according to Marc-Wogau, the primary sense of 'This table' is that in which it applies to a *Bestimmungskomplex* in which the local and the temporal predicate-constituents are both completely determinate. The sense in which it applies to the table as an enduring continuant with a variegated history is derivative from this.

(ii) According to Marc-Wogau, it is *only* the local and the temporal predicate which must be completely determinate when such a phrase as 'This table' is used in the primary sense. The other predicates which are either explicit or implicit constituents of such a *Bestimmungskomplex* may be, and generally are, incompletely determinate.

Now Marc-Wogau identifies a *Particular Individual* with a *Bestimmungskomplex* of the kind just described, *i.e.*, with one which contains as constituents a perfectly determinate local and temporal predicate and as other constituents other kinds of predicate which need not be completely determinate.

He proceeds to compare this account of particulars with others that have been given by eminent philosophers, *e.g.*, Russell, Stout, and Johnson. I do not propose to consider these comparisons in detail; but it will be worth while to notice certain statements which occur in them, because they throw light on Marc-Wogau's own theory.

(i) It has been said that universals and particulars may be distinguished by the fact that universals *have* instances, whilst

particulars *are* instances of universals but do *not* have instances. Marc-Wogau rightly says that, if we are to make use of this, we must clear up the notion of 'instance'. He then embarks on a long and difficult discussion which occupies pages 170 and 171 of his book.

It seems to me that this discussion would be greatly simplified if one began by pointing out that the word 'instance' is used in the following two senses. (*a*) It would be in accordance with usage to say: 'Scarlet is an instance of red' or 'An oak is an instance of a tree'. In this sense 'instance of' means 'determinate under' or 'species of'. (*b*) It would also be in accordance with usage to remark, on seeing a red flash or pointing to a tree, 'That is an instance of red' or 'That is an instance of a tree', as the case might be. Here 'instance of' means 'determinately dated and localised occurrence of'. A universal has instances, in the sense of sub-species or determinates under it, unless it happens to be completely determinate. It has, or may have, instances, in the sense of determinately dated and localised occurrences, whether it is or is not completely determinate.

Now it is true that a determinately localised and dated occurrence of scarlet, *e.g.*, is *ipso facto* a determinately localised and dated occurrence of red, just because scarlet is a determinate under red. It is also true that each is a *particular*. But it does not follow that a particular can have instances in either of the two senses in which a universal can do so. Scarlet-here-now is not an instance of red-here-now, either in the sense in which scarlet is an instance of red, or in the sense in which scarlet-here-now is an instance both of scarlet and of red. For scarlet-here-now is not a *species* of red-here-now; it is not a species of, or determinate under, *anything*. And it is not a *determinately localised and dated occurrence* of red-here-now; it is such an occurrence of *scarlet* and equally of *red*. I think it is plain, then, that Marc-Wogau is right in saying that, on his view of particulars, a particular would not have instances in either of the two senses in which a universal may have instances.

(ii) In comparing his view with the doctrine, asserted by both Johnson and Russell, that a distinguishing mark of a particular is that it can function in a proposition only as a *logical subject*, Marc-Wogau makes the following remark. There is, he says, a fundamental question which can be raised about both these philosophers. Do they regard a logical subject as (*a*) a complex whole in which its predicates can be distinguished as constituent factors, or (*b*) something which remains over when one abstracts from all its predicates? He says that, on the first alternative,

their view need not be (though of course it might be) fundamentally different from his own, according to which a particular is a *Bestimmungskomplex* containing a completely determinate local and temporal predicate. On the second alternative, he says, there would be a radical difference.

Now it seems to me that Marc-Wogau has not distinguished enough alternatives here. I should have thought that at least the following three need to be distinguished. (1) That a particular is a complex whole whose *only* elements are its predicates. (2) That it is a complex whole which contains its predicates as elements, but *also* contains an element of an entirely different kind from any predicate. (3) That, whether it be complex or not, it does not contain any of its predicates as elements. I take it that Marc-Wogau's theory is a form of *Alternative 1*. A person who accepted *Alternative 2* might hold either (*a*) that the non-predicative element in a particular *could* occur in isolation, or (*b*) that it could *not*. It should be noted that a person who held *Alternative 2* would be very liable to use the word 'particular' or 'substance' ambiguously. He might use it sometimes to mean the *complex whole* composed of the predicates and the non-predicative element, and sometimes to mean the *non-predicative element* in such a whole. He would be specially liable to make this confusion if he held *Alternative 2* in the *Form b*, *i.e.*, if he thought that the non-predicative element in a particular cannot occur except in combination with predicates to form a particular. We might avoid this ambiguity by using the word 'hypostasis' for the alleged non-predicative element, and the word 'substance' or 'particular' for the complex whose elements are an hypostasis and a number of predicates.

I take it that, on Marc-Wogau's view, a particular might be symbolised by some such formula as $R(P_1, P_2 \ldots; s, t)$. Here P_1, P_2, etc., represent predicates, other than those of local and temporal position, which may be relatively indeterminate. The letters s and t represent respectively a perfectly determinate spatial position and a perfectly determinate date. They are separated from P_1, P_2, etc., by a *semi-colon* to show that they perform a unique function in the complex, quite unlike that of the other predicates. The R outside the bracket represents a unique mode of relationship or form of union or 'bond' or 'tie', which I have called 'compresence'.

Alternative 2 would be represented by some such formula as $S(\theta; P_1, P_2 \ldots; s, t)$. Here θ represents the non-predicative element which I have called an 'hypostasis'; and S represents a bond between factors of three different kinds, *viz.*, the

hypostasis, the non-spatio-temporal predicates, and the local and temporal positional predicates.

(iii) It has been said that a particular is something which is completely determinate in every respect. Marc-Wogau has to consider how this is related to his doctrine that a particular is a *Bestimmungskomplex* in which only the local and temporal predicates need be completely determinate.

Let us simplify the question at the outset, as Marc Wogau does, by confining our attention to the case of a visual prehensum. It does seem to me that a visual prehensum must *have* a certain completely determinate colour, shape, extension, etc., as well as a certain completely determinate date and position in a certain visual field. In having these determinates it will, of course, *ipso facto* have all the less determinate predicates under which any of them fall. But it seems to me unintelligible to suggest that a determinable predicate could be manifested at a certain time and place without being there and then manifested in a certain perfectly determinate form. On the other hand, I have no difficulty in admitting that a prehended particular, *e.g.*, a visual prehensum, may be, and perhaps always is, *prehended as having* characteristics which are not completely determinate. One could say that it is 'circular or nearly so', 'scarlet of a light shade and considerable intensity' and so on; but one could not go further than this. (In the case of colours and of most shapes we have no names for perfectly determinate forms. So, even if one prehended a particular as having a certain perfectly determinate colour and shape, one could not express in words anything more than a certain relatively indeterminate colour and shape under which these determinates fall.)

Before concluding this sub-section I will make some general remarks about the theory of particulars of which I take Marc-Wogau's theory to be an instance.

(1) It has its maximum plausibility when confined to visual prehensa. It is not unplausible to hold that, when we say that a scarlet visual prehensum exists, we mean simply that scarlet is now being manifested in a certain position in a certain visual field. But even in this case the expression 'scarlet-here-now', which is often given as the right translation, is terribly inadequate. 'Here', *e.g.*, does not distinguish between one position in a visual field and other simultaneous positions in it. A scarlet and a blue visual prehensum, or for that matter two scarlet visual prehensa of precisely the same shade, shape, and size, may co-exist in the same visual field. Again, they may exist in different contemporary visual fields.

(2) Then something must be said about the shape and extension of a visual prehensum. At the very least it must be described as a certain *area* in a certain *position* in a certain visual field pervaded by a certain colour during a certain period. When all this has been added one wonders whether the visual field is not being treated as a kind of hypostasis; a kind of Substantival Absolute Space waiting ready to be 'inhered in' here or there by this or that colour. Perhaps the answer that would be made is that, while 'inherence' in general is a vague unintelligible term, the notion of the *pervasion* of an area of a visual field by a colour is clear and intelligible.

(3) The complication increases when we pass from particulars to which only one kind of predicate, *e.g.*, colour, is ascribed to particulars which are described, *e.g.*, as brown and cold and hard and round. What is the analysis of 'compresence' when these very different kinds of predicate are said to be compresent in a certain perfectly determinate position at a certain date? And what kind of space is it that houses these very different predicates?

(4) So far we have spoken only of those predicates which are *qualities*, *e.g.*, redness, coldness, etc. But a very important class of predicates in the case of a material thing or a person are its *dispositional properties*, *e.g.*, its melting-point, its density, its electrical conductivity, and so on. Can the inherence of these in a particular be dealt with on the same lines as that of pure qualities, *viz.*, by talking of 'compresence' in a certain position at a certain date? And, if so, will not the notion of compresence need to be modified still further?

(B) *Identity and Difference.* Marc-Wogau discusses this subject from page 90 to page 102. He begins by distinguishing three kinds of difference, which he calls *numerical*, *qualitative*, and *abstractive*.

Numerical difference can be predicated when and only when we can properly speak of two or more objects.

He gives three examples of *qualitative difference*. (i) Two *predicates* can be said to be qualitatively different if they cannot both at the same time and in the same respect be applied to the same subject. 'Red all over' and 'blue all over' would thus be qualitatively different predicates. (ii) Two *Bestimmungskomplexe* may be qualitatively different. The examples given are 'red sphere', 'green sphere', and 'red cube'. Suppose we substitute for the phrase 'red sphere' the equivalent and more explicit phrase 'redly coloured and spherically shaped surface', and make similar substitutions, *mutatis mutandis*, in the two other

cases. Then it becomes plain that any two such *Bestimmungskomplexe* are qualitatively different if any determinable which is an element in both is present in different determinate forms in the two. (iii) More elaborate examples of qualitative difference between *Bestimmungskomplexe* are the following. 'Spot consisting of a white circle surrounded by a black ring' and 'Spot consisting of a black circle (of the same size as the white one) surrounded by a white ring (of the same size as the black one)'.

Abstractive difference also covers several cases. It may apply either (1) to universals, or (2) to particulars. In the case of universals there are two possibilities. (1.1) The example here is 'triangular' and 'equilaterally triangular', *i.e.*, a relatively indeterminate predicate and the same predicate specified by a differentia. (1.2) The example here is 'three-sided figure' and 'equal-sided figure'. Here we have the same relatively indeterminate predicate specified by two different differentiae which are not *qualitatively* different, because not incompatible with each other. (I can see that this is not an instance of qualitative difference, as defined by Marc-Wogau; but it is not clear to me that it is appropriate to call it an instance of 'abstractive' difference.)

(2) The example which Marc-Wogau gives of abstractive difference as applied to particulars is important. Suppose that a cube is lying on a table with a certain face F_6 downwards and a certain other face F_3 upwards. It is observed from opposite sides of the table by two observers X and Y. X can see the faces F_1, F_2, and F_3 and no others; Y can see the faces F_3, F_4, F_5 and no others. It is assumed that the positions of the observers are such that what each prehends is prehended by him neither as distorted in shape nor foreshortened in size. Marc-Wogau says that the two prehend numerically different objects, but that these objects differ only abstractively. Each object has certain features which the other lacks; but there would be no inconsistency in supposing that all the features which belong to either were compresent in a single object.

This example should be contrasted with the case of two observers who look at the same penny, one from a great distance and in a very oblique direction, and the other from a few feet away and in a direction which is exactly or nearly at right angles to its surface. Here also the two prehend numerically different objects, but now the objects differ qualitatively and not only abstractively. The one object is prehended as round and unforeshortened; the other as elliptical and 'unnaturally small'.

It would be inconsistent to ascribe roundness and ellipticity of contour, or two different extensions, to a single object.

It should be noted that Marc-Wogau insists that, when two contemporary particulars are said to differ only abstractively, it is assumed that the determinate spatial position is the same in both of them. If one occupies a different position from the other, this constitutes *ipso facto* a qualitative difference between them.

Lastly, Marc-Wogau asserts that either qualitative or abstractive difference entails numerical difference; but neither qualitative nor abstractive difference entails the other. (It would appear from the examples that qualitative difference in a *given respect* excludes abstractive difference in *that respect*. But neither excludes the other in a different respect.)

If I am not mistaken, the notion of abstractive difference, as applied to particulars, can be illustrated as follows. Let P, Q, . . . represent characteristics, which may be incompletely or completely determinate. Let p, q, . . . represent characteristics which are determinates under P, Q, . . . respectively when the latter are not completely determinate. Let s represent a perfectly determinate spatial position, and t a perfectly determinate date. Consider the following sets of *Bestimmungskomplexe*. (i) P-at-s-at-t and (P & Q)-at-s-at-t. *E.g.*, brown-here-now and (brown-and-round)-here-now. (ii) (P & Q)-at-s-at-t and (Q & R)-at-s-at-t. (It is assumed here that there is no incompatibility between P & Q and R, and no incompatibility between Q & R and P.) *E.g.*, (brown-and-round)-here-now and (round-and-cold)-here-now. (iii) (P & Q)-at-s-at-t, (p & Q)-at-s-at-t, (P & q)-at-s-at-t, and (p & q)-at-s-at-t. *E.g.*, red-and-triangular)-here-now, (scarlet-and-triangular)-here-now, (red-and-equilaterally-triangular)-here-now, and (scarlet-and-equilaterally-triangular)-here-now. In each of these three sets, according to Marc-Wogau, we have *two or more* particulars, which differ only *abstractively*.

Now what seems to me odd is to assert with conviction that in all such cases there are two or more particulars. This is of course entailed by saying that abstractive difference involves numerical difference. It seems not unplausible to say that there are two particulars in the example of the objects visually prehended by the two observers of the cube. But it seems to me most paradoxical to say this, *e.g.*, in the example of (red-and-triangular)-here-now and (scarlet-and-equilaterally-triangular)-here-now. Surely the ordinary way of formulating this situation would be to say that there is a *single* particular, which is *less determinately*

described in the one case and *more determinately described* in the other. It is not clear to me that Marc-Wogau is logically obliged by his account of particulars to say that there are two or more particulars in all such cases. But, if that is a logical consequence, I should be inclined to regard it as a *prima facie* objection to his account of particulars.

We can now pass to Marc-Wogau's account of ' one and the same ', as applied to particulars where there is no question of difference of date. (The case of identity through time has to be considered separately.)

(1) We may use the phrase to deny *numerical* difference. We may mean to assert, *e.g.*, that, although there are two different descriptions, yet there is just one object answering to both. (As I have said, it seems to me that many cases which Marc-Wogau would count as instances of numerical difference with mere abstractive difference fall much more naturally under this head.)

(2) We may intend to deny *certain qualitative* differences between two objects. I think that a good example of this would be when we call two simultaneous occurrences of the same written or spoken type-word ' the same word '. Certain likenesses between two tokens of the same type-word are so much more important than the unlikenesses, for everyone but the typographer or the phonologist, that we commonly ignore the latter.

(3) We may intend to deny *all qualitative* difference, but not abstractive difference ; and therefore, according to Marc-Wogau, not numerical difference. He says that the objects prehended by the several observers of the cube in the above example, and also the *cube itself* (assuming that their visual experiences are veridical and that they really are both seeing a certain cube), are ' one and the same object ' in this sense.

Of course all parties are agreed that, in this example, there is a sense in which it is correct to say that both are ' seeing the same object ', *viz.*, a certain cube, and a sense in which it is correct to say that they are seeing partly different objects, *viz.*, different parts of the surface of that cube. Marc-Wogau admits that it is possible to give a different account of this situation from that which he has given. *E.g.*, it might be said that the different objects which the two observers *visually prehend* are alike in nature, but that they differ fundamentally in nature from the one cube which they both *see*. And it might be held that each of the former stands to the latter in a certain determinate form of the same peculiar relation, *viz.*, that of being *an aspect* or *appearance* of it. That is the kind of account which most

Sense-Datum Theorists would give. Marc-Wogau thinks that their reasons are inconclusive, and wishes to substitute the account given in *Sub-section B* of the *Introduction* of this paper.

(4) Two objects may be called 'one and the same' when we do not mean to deny even qualitative difference between them. Take, *e.g.*, the example of the two persons looking at a penny lying on the table. The objects prehended respectively by the near-by observer who looks straight downwards on the penny and the distant observer who views it obliquely differ qualitatively and therefore numerically. But the two observers would be said to be seeing 'the same part of the same penny'.

It seems to me that all that Marc-Wogau is really entitled to say at this stage of the discussion is the following. We certainly have two different *descriptions, viz.*, 'the object visually prehended by *A*' and 'the object visually prehended by *B*'. Let us abbreviate these to '*A*'s visual prehensum' and '*B*'s visual prehensum'. The following propositions are certainly true. (i) *A* prehends his prehensum as round. (ii) *B* prehends his prehensum as elliptical. (iii) One and the same object cannot at the same time have a round and an elliptical contour. Now, *if and only if* it is assumed that a visual prehensum must have any quality which it is prehended as having, it follows that *A*'s visual prehensum differs qualitatively from *B*'s. And in that case it follows that the two descriptions '*A*'s visual prehensum' and '*B*'s visual prehensum' describe two different objects, *i.e.*, that we have a case of numerical difference.

But, unless this assumption is made (and Marc-Wogau discusses it elaborately elsewhere and seems inclined in a certain sense to reject it), there is no certainty that we have a case of qualitative and numerical difference here. Why not say that there is literally *one* particular, visually prehended by both *A* and *B*; but that at least one of these observers visually prehends it as having a characteristic which is other than and incompatible with a characteristic which it actually has?

Be that as it may, Marc-Wogau thinks that this is a clear case of two qualitatively different objects being called 'one and the same'. He thinks that the phrase 'one and the same', when used in this sense, has no conventional connotation. The most that we can do is to mention certain conditions, one or more of which is generally fulfilled when the phrase is felt to be appropriate. He thinks that the most important of these are qualitative likeness, identity of position, and a common causal ancestor. But he says that any one of them may fail and yet the phrase 'one and the same' may be applied, and that any of them may

hold and yet the phrase be withheld. He gives various examples to illustrate this.

He points out that cases occur where two of these conditions are fulfilled but there is something which positively conflicts with the remaining one. Examples are the two objects visually prehended when one ' sees double ' ; the two objects prehended by different observers of the same ' ambiguous figure ', to whom it appears in characteristically different ways ; and so on. In such cases common-sense is uncertain whether to affirm or to deny ' sameness '.

(C) *Existence.* Marc-Wogau discusses the notion of Existence from page 67 to page 85. After a long discussion he distinguishes three senses in which ' existent ' or ' non-existent ' or some equivalent pair of opposites can be applied to particulars.

(1) In the widest sense one says of any prehensum that it ' exists ' so long as it is being prehended. This is regardless of whether it is prehended in a dream or hallucination or illusion or in normal waking perception. Marc-Wogau denotes this sense of ' exist ' by E_1.

(2) Among objects which E_1 we distinguish those which are prehended in normal waking perception from those which are prehended in dreams, delirium, or hallucinatory waking perception. The former are said to ' exist ' or to be ' real ' ; the latter to be ' non-existent ' or ' unreal '. The former would include both the visual prehensum of the person who sees the penny as round and of normal size, and that of the person who sees it as elliptical and foreshortened. This sense is denoted by E_2.

(3) If a prehensum E_2 its prehended characteristics may differ either (i) only abstractively, or (ii) qualitatively, from the characteristics of the actual visum. In the former case we say that it ' exists in the strict sense '. The visual prehensum of the near-by observer who looks straight down on the penny exists in this sense ; that of the other observer does not. This sense is denoted by E_3.

Marc-Wogau remarks that E_1, E_2, and E_3 may not be three different *meanings* of the word ' exist '. It may be that the word, in each of its applications, has no conventional connotation ; and that all that can be done is to describe in each case the conditions under which it would commonly be applied or withheld. The conditions for applying E_1 are simple. It is applied at any time to all those objects and only those which are then being prehended. E_2 is applied when and only when it is believed that an important factor in causing the visual experience was either the actual visum or some other material object connected

with the latter in certain determinate ways. (I suppose that the latter alternative is intended to cover the case of such objects as mirror-images, double-images, etc.)

The conditions under which E_3 is applied are much more complicated. Marc-Wogau enumerates the following, without claiming that his list is exhaustive. (i) Congruence between sight and touch. (ii) Constancy of the visual appearances obtained under certain conditions (*e.g.*, normal illumination) in spite of variations in other conditions. He admits that there are many exceptions and qualifications to be made here. I should have thought that appropriate *variation* of the appearances concomitantly with certain variations in the conditions was an equally important criterion for E_3. (iii) Certain causal relationships to other objects already believed to E_3.

Marc-Wogau raises here the following logical question. Suppose that one could enumerate a rather large number of conditions $C_1, C_2 \ldots C_n$, such that if *any* of them were believed to be present we should apply 'E_3' and if *all* of them were believed to be absent we should refuse to apply it. Could we then say that 'E_3' has a *meaning*, and that its meaning is the alternative characteristic C_1-or-C_2-or $\ldots C_n$? Marc-Wogau thinks not. He holds that we should say that 'E_3' has a meaning only if we thought that there is either a single non-alternative characteristic C or a conjunction of such characteristics C_1 and C_2 and $\ldots C_n$, such that 'E_3' is correctly applied when and only when C is believed to be present or C_1 & C_2 & $\ldots C_n$ is believed to be present, as the case may be.

Now he thinks it most unlikely that there should be any *quality* or set of qualities answering to these conditions. But might there not be a *relational property* or a set of them, answering to these conditions, which would constitute the meaning of 'E_3'?

He finds the following difficulty in this suggestion. On this supposition what is meant by ascribing E_3 to an object O is either (i) that O stands in certain relations to certain other objects taken severally, or (ii) that it stands in certain relations to a certain set of interconnected objects taken collectively. Now he thinks that it would always be necessary to add the proviso that each of these objects, or this system of interconnected objects, as the case may be, exists. If 'exists' here means the same as 'E_3', we have a logical circle in the attempted definition of 'E_3'; if it does not, we have a fourth sense of 'existence' on our hands. (This does not seem to me to be certain. Might not the sense required be E_1 or E_2? Would not a Berkeleian or a Phenomenalist, *e.g.*, say that it was E_1?)

On the whole Marc-Wogau is inclined to think that 'E_3' is a *label* which is 'meaningless' in the following sense, *viz.*, that there is no set of non-alternative characteristics such that it would be appropriate to apply it if and only if all of them were believed to be present. This label is commonly tacked on to a prehensum when any one, or any selection, of a number of alternative but not mutually exclusive conditions is believed to be fulfilled. He compares this to the process of marking certain material things with a cross in order to distinguish them from others. (There are generally reasons for this in any particular case. *E.g.*, a frequent reason is that the things belong to a single person or that they are to be sent to a certain place.) When certain things have been so labelled they *ipso facto* acquire a common and peculiar property, *viz.*, that of being marked with a cross. Similarly, when certain prehensa have, for one reason or another, had the adjective 'E_3' attached to them, they *ipso facto* acquire the common and peculiar property of being said to 'E_3'. But this cannot be described as the meaning of 'E_3', in the sense in which, *e.g.*, the meaning of 'circle' is line all of whose points are equidistant from a fixed point.

(D) *Material Things.* Under this head Marc-Wogau discusses two general problems, *viz.*, (1) the Self-Identity of a thing through time, and (2) the Unity of a thing in relation to its many contemporary aspects.

(1) *Self-identity through Time.* The discussion of this problem occupies pages 2 to 16 of Marc-Wogau's book. I think that the following is the best way to introduce it. We have two definite descriptions, referring to two different moments of time, *e.g.*, (i) 'The material object which occupied a certain place five minutes ago' and (ii) 'The material object which occupies a certain place (it may be the same or a different one) now'. Sometimes we say or imply that these are just two descriptions of a single object. *E.g.*, 'the thing that was in the middle of my writing-table five minutes ago is in my hand now'. Sometimes we say or imply that each is a description of a different object. Under what conditions do we say that there is a *single* object answering to two such descriptions?

Let us state the question in the following form. Under what conditions do we say that there is a single thing answering to two such descriptions as 'The material object which occupies s_1 at t_1' and 'The material object which occupies s_2 at t_2', where s_1 and s_2 may be either the same or different, but t_1 and t_2 are assumed to be different?

I think it is plain that there would be a number of different

cases to be considered if the question were to be treated fully. We may say that a material thing has *remained at rest* at s_1 throughout the interval from t_1 to t_2 or that it has *moved*. If it has moved, it may have moved *continually* throughout the interval or it may have *stopped* from time to time. In either case it may have occupied any one position *only once* or it may have occupied one or more positions *several times*. (Several of these possibilities are illustrated by the case of a swinging pendulum.) Marc-Wogau, quite reasonably, refrains from mentioning these tiresome complications of detail. We will confine ourselves to the following two cases. (i) Where a material object is said to have remained *at rest* at s_1 from t_1 to t_2. (ii) Where it is said to have *moved continually* and *without occupying any position more than once* from s_1 at t_1 to s_2 at t_2.

If the question is formulated in this way, I think that Marc-Wogau's answers for the two cases would be as follows. *Case I.* Here one would be inclined to say that there is a single thing answering to the two descriptions 'The material object which occupies s_1 at t_1' and 'The material object which occupies s_1 at t_2' if one believed (rightly or wrongly) that the following conditions were fulfilled. (i) That for every moment t between t_1 and t_2 there is a particular answering to the description 'The material object which occupies s_1 at t'. (ii) That all such particulars are exactly or predominantly alike in their qualities. (iii) That, if there are differences of quality, it is true on the whole (though there may be occasional exceptions) that particulars of this set which are nearer to each other in time are more alike than those which are further apart in time.

Case II. Here one would be inclined to say that there is a single thing answering to the two descriptions 'The material object which occupies s_1 at t_1' and 'The material object which occupies s_2 at t_2' if one believed (rightly or wrongly) that the following conditions were fulfilled. (i) That for every moment t between t_1 and t_2 there is a particular answering to the description 'The material object which occupies s at t', where (a) for each value of t there is a different value of s, and (b) the values of s corresponding to any two values of t differ by an amount which tends to zero as the difference between the values of t tends to zero. (ii) and (iii) are the same respectively as (ii) and (iii) in Case I.

The following points should be noticed about these answers.

(i) What is relevant is what the person who asserts or denies identity through time *believes* to be the case, not what really is the case if that should differ from what he believes.

(ii) It is plain that, in accordance with Marc-Wogau's general account of particulars, we are concerned with a series of numerically different particulars, even when we correctly and truly talk of 'the same material thing'. For, according to that account, two descriptions of a particular, into which there enter different moments of time, are inevitably descriptions of different particulars.

(iii) It will be noticed that, in stating the theory, I have talked throughout of 'the *material object* which occupies s at t' and not of 'the *particular* which occupies s at t'. I have done this deliberately for two reasons. In the first place, if I have understood Marc-Wogau's theory of particulars aright, there could be a plurality of particulars occupying the same place at the same time. If so, there would be nothing answering to the description '*the* particular which occupies s at t'; for that implies that there is *only one*. Secondly, I understand Marc-Wogau to hold that it is impossible to describe the difference between a series of particulars which *does*, and one which does *not*, constitute a material thing without bringing in the notion of 'material thing'. I understand him to assert that all attempts to do this by means of the intrinsic peculiarities of the former kind of series are either inadequate or circular.

Marc-Wogau lays considerable stress on the following feature of his theory. Although he has mentioned several conditions under which a person would be inclined to say that two descriptions, involving different moments of time, both apply to 'the same thing', he does not wish to assert that these conditions constitute the *meaning* of the phrase 'same thing at two different moments'. So far as I can understand, he is inclined to deny that this phrase has a meaning, in the sense in which, *e.g.*, the word 'circle' has. In this view he associates himself with the Uppsala philosopher Phalén. This may be compared with the doctrine, already mentioned, that 'existence in sense E_3' has no meaning but should rather be compared to a label.

If I may state what I suppose to be Marc-Wogau's view on this point in my own way, I think that it comes to the following. Suppose that m_1 answers to the description 'the material thing which occupies s_1 at t_1' and that m_2 answers to the description 'the material thing which occupies s_2 at t_2'. Then (i) if a person believes that m_1 and m_2 are members of a series of successive particulars in which every one of a certain set of properties P_1, P_2, . . . P_n are present, he will be *inclined as a rule* to call m_1 and m_2 'the same thing'. (ii) Unless he believes m_1 and m_2 to be members of such a series he will *not as a rule be inclined* to

call them 'the same thing'. (iii) If he positively believes that all series of which m_1 and m_2 are members lack one or more of these characteristics, he will be *disinclined as a rule* to call them 'the same thing'. But there is *no* set of properties $P_1, P_2, \ldots P_n$ such that (i) a person who believed that m_1 and m_2 are members of a series having all these properties would be *speaking incorrectly* if he said 'm_1 and m_2 are *not* the same thing'; and such that (ii) a person who believed that every series of which m_1 and m_2 are members lacks one or more of these properties would be *speaking incorrectly* if he said 'm_1 and m_2 *are* the same thing'.

In view of the difficulty of finding any set of properties which could be taken as the conventional connotation of the phrase 'same thing at different moments'—a difficulty which is well brought out by Marc-Wogau's examples—I think that it is probably wise to state the theory in this carefully guarded way.

There is one other point to be noticed before we leave this topic. Marc-Wogau raises the question whether there is not a logical circle in making continuous spatio-temporal connexion between the material thing which occupies s_1 at t_1 and the material thing which occupies s_2 at t_2 a condition for calling them 'the same thing'.

The difficulty may be put as follows. Suppose that m_{12} and m_{23} are adjoined in time and that they occupy the same or adjoined positions in space. Can we attach any meaning to the latter part of this statement except the following? (i) That m_{12} stands to a certain other material thing M_{12} (which is contemporary with it) in a spatial relation which is the same as, or only slightly different from, that in which m_{23} stands to M_{23} (which is contemporary with *it*); and (ii) that M_{12} and M_{23} are called 'the same material thing'. If so, will not precisely the same question arise about M_{12} and M_{23}, *viz.*, what induces us to call *them* 'the same material thing'? If we give the same kind of answer, we shall be referred to M_{12}' and M_{23}', and the same question will arise about *them*. And so on to infinity.

Marc-Wogau's answer is that each of us has a sense of position and orientation which is independent of reference to his own or to other bodies. Each of us is aware of surrounding space as a 'structural whole', in which he can distinguish determinate positions even when no external object is visible, and can recognize them again even when he has moved. In particular one recognizes the distinction between vertical and horizontal independently of the orientation of one's body at the time. It is true that the distinction *left-right* and *behind-or-in-front* are essentially relative to one's body. Yet even in that case a rotation

of one's body is experienced as a movement of it within a fixed surrounding space. After having turned through an angle with one's eyes shut one can still indicate the direction, relative to the present orientation of one's body, in which an object, formerly seen straight in front of one, now lies. For these reasons Marc-Wogau thinks that the objection of circularity fails.

I have no wish to deny either the alleged facts or their importance for the phenomenology of spatial perception. But I wonder whether they constitute an answer to the original difficulty. That is concerned with *physical* things in *public* space, and not, *e.g.*, merely with the location of visual prehensa in the visual fields of those who prehend them. Now, in spite of the phenomenological facts which have been adduced, does it not remain true that the location of physical things and events in public space is accomplished only by referring them to certain *bodies* chosen as a system of reference ?

(2) *Unity of a Thing in relation to the Plurality of its Contemporary Aspects.* Consider the examples of the two persons who look at the same cube from opposite sides of the table ; and the two persons who look at a penny, one from near-by and perpendicularly, and the other from a great distance and in a very oblique direction. There is a sense in which the two observers in each case *see different objects.* But we also use language which seems to imply that they *see the same object ;* for we say that the two observers in the first example ' see the same cube ' and in the second ' see the same penny '.

The problem presented by these examples can be put in either of the following alternative forms. (i) How can two different co-existent objects, *e.g.*, the faces F_1, F_2, and F_3 and the faces F_3, F_4, and F_5 of the cube, or the round-looking unforeshortened-looking object and the elliptical-looking foreshortened object, be one and the same thing, *viz.*, a certain cube or a certain penny ? (ii) How can we say that two observers ' see the same thing ' when we admit that they see different objects ?

Marc-Wogau says that the problem is most acute in such cases as the example of the two observers of the penny. In the cube-example common-sense would accept the answer that, in the strictest sense of ' see ', each observer sees only a *part* of the cube, and that the parts seen by the two only partially overlap. In the case of an object seen both directly and in a mirror common-sense would admit that what is expressed by saying ' I saw such and such an object in the mirror ' would be more accurately expressed by saying ' I saw in the mirror a reflected image of such and such an object '. But common-sense is not inclined to admit

that it would be more correct to say that the remote observer of the penny from a very oblique direction saw a very small elliptical object. It insists on saying that he saw the same round moderate-sized object as was seen by the other observer, but that it 'looked elliptical and foreshortened to him from where he was standing'.

The essence of Marc-Wogau's analysis is to be found on pages 102 to 118 of his book. It may be stated as follows. The phrase 'the thing *T* at the moment *t*' is applied, not to a *single* particular, nor to a certain set of inter-related particulars taken as a *collective whole*, but *distributively* to every one of a certain class of particulars. This class may be subdivided into two parts, which may be called the 'nucleus' and the 'fringe'. For the present we will confine our attention to the nuclear members. The fundamental feature of these is that they differ from each other *only abstractively*. (The objects prehended by the two observers of the cube would be nuclear. The object prehended by the person who looks at the penny very obliquely from a great distance would be non-nuclear.)

Marc-Wogau considers two alternatives to this analysis. (i) One is that the thing should be identified with such a set of abstractively different particulars *taken collectively*. He points out that, on this analysis, it must be admitted that a person can never see a thing in the sense of *visually prehending* it. Such a sentence as 'I see a cube' will have to be translated into something like 'I visually prehend a certain member of a certain set of particulars which differ only abstractively from each other'. Marc-Wogau does not profess to refute this suggestion. But he thinks that it should be avoided, if possible, because it forces us to put such an out-of-the-way interpretation on many facts which are *prima facie* quite straightforward.

(ii) It might be alleged that the thing is something more concrete than any of these objects, and something to which they all stand in a common relationship. It might therefore be suggested that the phrase 'the thing *T* at the moment *t*' applies only to the one *completely concrete* member of a certain set of particulars which differ only abstractively from each other. To this Marc-Wogau answers that such a set of more and more concrete objects which differ only abstractively from each other is in principle infinite. So the notion of a thing as the one completely concrete member of such a set would be the notion of a *common upper limit* to a number of series which all converge on it, and not the *common last term* of all such series.

I suppose that Marc-Wogau's objection to this is that any 'material thing' would then be a kind of Platonic Idea, laid up

in Heaven, and once more not the kind of object that could be ' seen ', in the sense of visually prehended.

To this one might be inclined to retort as follows. Why not say that the material thing is the one actual concrete existent in connexion with such a set of particulars, and that the other members of the nucleus are all abstractions from it in the following senses ? (i) Each is only a *part* of its *surface*. (ii) Each is *visually prehended* only as having certain characteristics, though it in fact has others also which cannot be *visually* prehended or cannot be *prehended* at all. (iii) Each is visually prehended only as coloured, shaped, etc., in an *incompletely determinate* way, though in fact it is qualified completely determinately. This is at least less odd *prima facie* than to hold that there are particulars which *are* incompletely determinate in their qualities, and are not merely *prehended as* qualified more or less indeterminately.

We return now to the original suggestion that such a phrase as ' that penny there now ' applies distributively to every one of a certain set of particulars which differ only abstractively from each other. Marc-Wogau remarks that we can ascribe, in a certain sense, to each *member* of such a set the properties of *every* other member of the set, and not only those properties which it is prehended as having. Thus, *e.g.*, we can say that the object prehended by a person, who merely looks at a block of ice without touching it, is *cold*, although of course he does not visually prehend that object as cold. But all such statements are analysable on the following lines: ' This particular, which I visually prehend only as translucent and as a certain part of the surface of a cube (and which in fact has only those qualities), differs only abstractively from certain other particulars which co-exist with it in the same place and are cold '. We might sum up this part of the theory as follows. To *prehend* in any way any member of such a set of particulars is *ipso facto* to *perceive mediately* any other member of the same set, and therefore to perceive mediately all the more concrete members of that set.

Now I think it is important to contrast this analysis with the following *prima facie* possible alternative, which the terminology introduced at the beginning of this paper was meant to leave open. It might be suggested that the *very same* particular, which I visually prehend only as translucent and as a certain part of the surface of a cube, has also other properties, such as coldness and smoothness, which I cannot *visually* prehend ; and that it has them in the same literal and non-dispositional sense in which it has the translucency and the cubical contour which I visually prehend it as having.

It should be noted that Marc-Wogau's dictum that abstractive difference involves numerical difference does not suffice by itself to rule out this alternative. What is required in order to rule it out is the following premiss, *viz.*, that a characteristic which a particular is *not* prehended as having cannot belong to it in the same literal non-dispositional sense in which a characteristic which it *is* prehended as having does so. On that assumption a particular which is prehended as translucent, and is not prehended as cold or smooth, must be a different particular from any which would be prehended as cold and smooth if it were prehended at all.

It remains to mention the *non-nuclear* members of that set of particulars to each member of which the phrase 'the thing T at the moment t' is applied. We say that the remote observer who looks at the penny very obliquely is seeing the same penny as the near-by observer who views it from above. But here the former observer prehends his prehensum as having certain characteristics which are incompatible with those which the latter prehends *his* prehensum as having. The difference is *qualitative* and not merely abstractive. Associated with this is another difference. The nuclear members exist in the sense E_3; the non-nuclear members exist only in senses E_1 and E_2.

It does not appear to me that the last statement adds anything substantial. For the distinction between these two senses of 'existence' is not independently defined; it is introduced and illustrated simply by reference to the contrast between such cases as the two observers of the penny.

(*To be concluded.*)

II.—THE CONTINENTAL RECEPTION OF HUME'S *TREATISE*, 1739-1741.

By Ernest C. Mossner.

"Never literary Attempt was more unfortunate than my Treatise of human Nature. It fell *dead-born from the Press;* without reaching such distinction as even to excite a Murmur among the Zealots." When Hume made that comment in *My Own Life* in 1776, he was, in effect, sealing off any effort on the part of future commentators upon his philosophy to search out contemporary reactions. Even in a day of high controversy, it would seem that no specific remarks upon the *Treatise* were made until those of Lord Kames and Stewart in the 1750's, Reid and Oswald in the 1760's, and Beattie in the 1770's. In the meanwhile Hume had become famous—in some circles, infamous—for his subsequent publications in philosophy, ethics, political science, commerce, history, fine letters, and esthetics. It is not altogether surprising, therefore, to find that Hume was so startled and puzzled by the determined late onslaught of his brother Scots upon the *Treatise* that, by virtue of this new widespread notoriety, he came to disown it as a "juvenile work, which the Author never acknowledged." This repudiation, which he considered to be "a compleat Answer to Dr. Reid and to that bigotted silly Fellow, Beattie,"[1] appeared publicly in 1777 in an advertisement to the posthumous and authoritative edition of his works.

The lack of contemporary philosophical interest in Hume's first effort, a work to-day almost universally recognized as exhibiting his highest genius, unquestionably had the most profound influence upon his subsequent career as man of letters. For the problem, to his mind, was primarily literary rather than philosophical. And Hume took upon himself the burden of the blame for not having put the *Treatise* in such a form as could at least be understood, if not approved, by the general reading public. "Where a man of Sense mistakes my Meaning, I own I am angry," he acknowledged to Stewart privately in 1754; "But it is only at myself: For having exprest my Meaning so ill as to have given Occasion to the Mistake."[2] So if Hume spent more than a decade in the original composition of the *Treatise*, he passed

[1] Hume, *Letters*, ed. J. Y. T. Greig (Oxford 1932), II, 301.
[2] *Idem.*, I, 187.

XUM

upwards of two decades in rewriting and publishing its various sections as the *Enquiry Concerning Human Understanding* (1748), the *Enquiry Concerning the Principles of Morals* (1751), and *Of the Passions* (1757).

Before attempting to survey the early Continental reception of the *Treatise*, it might be well to review briefly the main facts of publication. The first two volumes, "Of the Understanding" and "Of the Passions," were published at London by John Noon in January, 1739; the third volume, "Of Morals," was printed for Thomas Longman and did not appear until the autumn of 1740. The edition was limited to 1,000 copies and, following the not uncommon practice of the period, was anonymous. After the publication of the first two volumes but before that of the third, Hume had sought to stimulate interest by bringing out in March, 1740, *An Abstract of a Book lately Published; Entituled, a Treatise of Human Nature, &c., Wherein the Chief Argument of that Book is farther Illustrated and Explained.* According to the title-page, this anonymous sixpenny pamphlet was "Printed for C. Borbet, at Addison's Head, over against St. Dunstan's Church, in Fleet-street." Long unnoticed by Hume scholars, this work is now definitely assigned to Hume himself by its modern editors, the late Lord Keynes and Mr. P. Sraffa; and the publisher is understood to be C. Corbet.[1] Originally designed as an article for the London periodical, the *History of the Works of the Learned*, the *Abstract* was brought out as a pamphlet after the appearance of a hostile review in that journal in November and December, 1739.

The review of the *Treatise* in the *History of the Works of the Learned*, which is well known to present-day students of Hume, was the only English review at a time when there were few periodicals devoted to serious literature other than theological. Yet if there was a temporary dearth of learned journals in England, there certainly was none on the Continent. These Continental journals, however, have hitherto apparently never been consulted for notices of the *Treatise*. Without pretending to have made a definitive examination of them, many to-day being exceedingly rare, I may yet say that such as I have been able to consult have proved rewarding. In the following pages I shall attempt to describe the foreign comment in more or less detail as the case requires. These notices and reviews will be divided into the two language groups of French and German.

[1] See *An Abstract of a Treatise of Human Nature, 1740: A Pamphlet hitherto unknown by David Hume*, reprinted with an introduction by J. M. Keynes and P. Sraffa (Cambridge, 1938), Introd.

I. Comment in French.

In a period when publication in France was seriously limited by the requirement of an official *imprimatur*, Holland was the centre of free French-language publication, particularly of periodical literature. Anglomania was beginning to arise, and English books were in demand everywhere. Before the modern practice of supplying free review copies of new publications to the periodicals, the periodicals themselves were normally compelled to purchase the books outright—an expensive business. On occasion, however, they might arrange to rent books for review purposes. But in either case, for convenience, they customarily kept paid correspondents in London to select and to write accounts of the most important new publications. One of the most influential and most prolific of these London correspondents or editors was Pierre Desmaizeaux. A French Protestant refugee, Desmaizeaux had migrated to England before the end of the seventeenth century; as a journalist and hack-writer, he became a member of the Royal Society and the personal friend of such liberal thinkers as Pierre Bayle and Anthony Collins. Desmaizeaux was on the London staff of two of the three French-language periodicals noticing the *Treatise* and was, moreover, personally acquainted with its author.

(a) *Bibliothèque britannique.* The *Bibliothèque britannique, ou histoire des ouvrages des sçavans de la Grande-Bretagne*, was printed at The Hague by Pieter De Hondt. In the "Literary News from London," in the October, November, and December, 1739, number, appears a notice of the *Treatise*, "published recently by an anonymous writer." The comment [1] is brief and unsympathetic: "This is a system of logic, or rather of metaphysics, as original as can be, in which the author claims to rectify the most ingenious philosophers, particularly the famous Mr. *Locke*, and in which he advances the most unheard-of paradoxes, even to maintaining that operations of the mind are not free."

The following number of the *Bibliothèque britannique*, that for January, February, and March, 1740, attributes both the *Treatise* and the *Abstract* to the author of a *Treatise on Ancient Painting*, recently published in London, George Turnbull. As given in the Dutch periodical the title of the pamphlet is: *An Abstract of a*

[1] *Bibliothèque britannique*, 40[1] (1739), 216. All foreign quotations are given in English translation. For certain facts in this and the previous section, I am indebted to the article by F. Beckwith, "The Bibliothèque Britannique, 1733-47," in *The Library*, 4th Series, XII (1932), 75-82.

late Philosophical Performance entitled a Treatise of human Nature; being an Attempt to introduce the Experimental Method of Reasoning into Moral Subjects: Wherein the Chief Argument and Design of that Book, which has met with such Opposition, and been represented in so terrible a Light; is further illustrated and explained: and the work is listed as issued by "*C. Corbett* at Addison's Head."[1] The name of the publisher has at once been corrected and again misspelled and the title considerably altered and expanded. This title does not appear on any of the three copies known to the modern editors, nor have I been able to discover any others. As has already been seen, Hume had at first hoped to publish the *Abstract* in the *History of the Works of the Learned* but, after the appearance of what he termed a "somewhat abusive" review in that journal,[2] brought it out instead as a pamphlet. It would, therefore, seem a reasonable conjecture that, perhaps in the heat of indignation over the manifest unfairness of that review, he had a few copies printed off with a title-page reflecting his indignation before he characteristically cooled off and toned it down. Possibly, also, he knew of other hostile reviews.

What can be said of the attribution of both the *Treatise* and the *Abstract* to Turnbull? Author of the *Treatise on Ancient Painting* mentioned in the *Bibliothèque britannique*, George Turnbull was also the author of a two-volume *Principles of Moral Philosophy*, published in 1740 by John Noon, publisher of Hume's *Treatise*. By a coincidence, it was under this same Turnbull at Marischal College, Aberdeen, that Thomas Reid first studied philosophy. For what it is worth, I can only conjecture further the following series of blunders on the part of the London correspondent of the *Bibliothèque britannique*. In receiving a copy of the *Abstract* from the bookseller, he may have been told that this is by the author of the *Treatise*, meaning the *Treatise of Human Nature;* but, confusing it with the preceding item on his list, the *Treatise on Ancient Painting*, he elaborated on that confusion as indicated above, until he conceived Turnbull to be the author of the two *Treatises* as well as of the *Abstract*. In any event from certain facts to be presented shortly, it does not seem likely that Desmaizeaux had a hand in the account in the *Bibliothèque britannique*.

(b) *Bibliothèque raisonnée*. Besides Desmaizeaux, the editorial staff of the *Bibliothèque raisonnée des ouvrages des savans de l'Europe* included J. Barbeyrac, A. B. de la Chapelle, W. J.'s

[1] *Bibliothèque britannique*, 40[2] (1740), 436.

[2] Hume, *Letters*, I, 38; *History of the Works of the Learned* (1739[2]), 353-90, 391-404.

Gravesande, L. de Jaucourt, P. Massuet, and J. Rousset de Missy. This Amsterdam periodical notices both the *Treatise* and the *Abstract* and reviews the *Treatise* at considerable length in two instalments. Appearing in the " Literary News from London " in the April, May, and June issue for 1739, the first mention of the *Treatise* is merely descriptive.[1] Yet it is of high interest because it is the first place where the name of the anonymous author is given correctly. The article opens: " A gentleman, named Mr. *Hume*, has published a *Treatise of Human Nature* . . ."; and, after a careful description of the two volumes in print, section by section, it concludes: " Those who demand the new will find satisfaction here. The author reasons on his own grounds; he goes to the bottom of things and traces out new routes. He is very original." Mr. Hume could hardly have written a better blurb himself!

Concerning the source of this early attribution of the *Treatise* to Hume, this much may be said with certainty. In a letter of February, 1739, Hume stated that " . . . I have endeavoured all I could to conceal my name; though I believe I have not been so cautious in this respect as I ought to have been." And thirteen months later, in March, 1740, he conceded: " I was also determin'd to keep my Name a Secret for some time tho I find I have fail'd in that Point." It is further evident from a letter as early as April 6, 1739, that he had revealed his authorship of the *Treatise* to other than the few personal friends who were already in on the secret. For that letter indicates that he had previously met Pierre Desmaizeaux, had sent him a copy of the *Treatise*, and was now writing to ask for a critique.[2] Hume's letter to Desmaizeaux had begun, " Whenever you see my Name, you'll readily imagine the Subject of my Letter "; and it would seem a fair inference that it was through the medium of Desmaizeaux that Hume's name first appeared in print as the author of the *Treatise of Human Nature*. It might also very well be that the friendly blurb in the *Bibliothèque raisonnée*, the only known contemporary account completely undismayed by the free-thinking implications of the *Treatise*, was from the pen of Hume's London acquaintance, Pierre Desmaizeaux.

Exactly a year after the original notice in the Amsterdam periodical, appeared the first section of a forty-nine page review of the *Treatise*. (The ways of learned journals have not greatly changed in the past two centuries!) But also in the " Literary News from London " in the same issue, the *Abstract* was noticed,

[1] *Bibliothèque raisonnée*, 22^3 (1739), 481.
[2] Hume, *Letters*, I, 27, 38, 29.

this time under its customary title : " Some people having found that the *Treatise of Human Nature* of Mr. *Hume* was a little too abstract, a brochure has been published to help them understand it." [1] This was the second time that Hume's name was publicly put to the *Treatise* and, if due to Desmaizeaux, that gentleman was clearly not in on the secret of the *Abstract* or, if he was, was determined to keep it. Curiously enough, throughout the review itself the name of the author goes unmentioned ; it is always " our author " or " the anonymous author " or " the English savant." This discrepancy may be accounted for by the not too difficult supposition that the second notice, like the first, was written by Desmaizeaux but that the review was by another hand. And it will be observed that the review is by no means so friendly as the two notices.

The first instalment of the review of the *Treatise*, though purporting to cover the original two volumes, deliberately restricts itself to Hume's reasonings on cause-and-effect ; and Book II, " Of the Passions," is no more than mentioned in the final paragraph. The second instalment, in the April, May, and June issue of 1741, covers Book III, " Of Morals," which has been published late in 1740.[2] In the eighteenth-century manner, the review is a combination of paraphrase and quotation ; but there are enough incidental critical remarks to reveal the personal reactions of the writer. After a wide preliminary survey of the contemporary state of metaphysics, the reviewer comments on the *Treatise* :

> This entire work, in general, is filled with original thoughts, which have all the merit of singularity. Perhaps it will be found that in wishing to investigate the inmost nature of things, the anonymous writer sometimes uses a language a little unintelligible to his readers. Again I fear that his paradoxes favour Pyrrhonism and lead to consequences that the author appears to disown. Metaphysics has its stumbling blocks as well as the other sciences. When it passes certain limits, it obscures the objects that it searches out. Under pretence of yielding only to evidence, it finds difficulties in everything.[3]

In connexion with Hume's reasonings concerning cause-and-effect, the reviewer opines, " This is pretty close to the manner in which *Sextus Empiricus* formerly reasoned in his *Hypotyposes*, Book III, Chap. III " ; and again on the same subject, he ventures to take issue with his author, by repeating over and over again that the resemblance of the future to the past is a matter-of-fact, that the effect is infallible because a cause which operates in the

[1] *Bibliothèque raisonnée*, 24[2] (1740), 481-82.
[2] *Idem.*, 24[2] (1740), 324-55 ; 26[2] (1741), 411-27.
[3] *Idem.*, 24[2] (1740), 328.

same manner must produce the same effect.[1] On Hume's reduction of belief to "a lively idea, related to or associated with a present impression," he exclaims in exasperation: "This is the definition which the author himself gives. It would be perfectly unintelligible if the entire course and connexion of the propositions which we have just reviewed had not been mastered. I do not know whether even with all that it will appear very evident and it ought not to be said here, *Fiat Lux.*" [2]

Finally the reviewer reverts to a theme which must have become depressingly familiar to Hume:

> There would be a hundred things to remark in our author, whether on the taste for Pyrrhonism which reigns in his manner of philosophizing; whether on the inconsistency of so many of the singular propositions that he has been pleased to accumulate; whether finally on the pernicious consequences that could be drawn from his principles. What is most offensive is the confidence with which he delivers his paradoxes. Never has there been a Pyrrhonian more dogmatic. It is not by doubting that he dares to substitute his speculations for the opinion of the greatest philosophers on the most abstract matters, as on the nature of our ideas, on extension, on space, on the vacuum, on identity, etc. It is not by doubting that he proceeds to maintain, for example, that it is false that everything which exists must necessarily have a cause of its existence; that we have no proof *a priori* of the existence of God; that the most ingenious mathematicians have no certainty of the truth of their discoveries until the learned public has approved them; and a number of other propositions as bold, to say nothing stronger. The author is on all this as positive as can be. The Locke's and the Clarke's are often, to his eyes, but paltry and superficial reasoners in comparison with himself; and, if it be permitted to speak his language here, it is easy to see that habit and custom have already so framed him to believe that he believes nothing except in a very lively manner. It is good, however, for the public to know that it is a young philosopher to whom they are indebted for the essays with which we have just entertained them. What cannot we expect from a genius so subtle and so profound when once age will have ripened his taste, and he will have had time to meditate anew the matters that he has rough-hewn? [3]

This is not unlike the substance of the review in the *History of the Works of the Learned* though, indeed, without its arrogant tone. The reference to Hume's youth might be derived from that source; for the chronology of publication makes it possible.

The first instalment continues with a half-promise to proceed at a later date to Book II, "Of the Passions," which, concedes the reviewer, seems to have "more clarity." "While waiting," he continues, "we ask pardon from the learned and ingenious author for the errors which have slipped by us in our desire to

[1] *Idem.*, 24² (1740), 337, 340-41. [2] *Idem.*, 24² (1740), 343-44.
[3] *Idem.*, 24² (1740), 353-54.

abridge his reasonings." And the review closes on an ironical note, commenting that the author

> has burst through the bounds of a *prudent scepticism* and has made too frequent use of those means of speaking so commonly used inappropriately, *'tis evident, 'tis certain, 'tis undeniable ;* but he expressly declares that, if this has happened to him, it is inadvertently in consequence of the lively impression that objects have made on him and not at all through egotism or a taste for *dogmatic philosophy*, from which he is, he says, *very remote*. And, in truth, one would have to be madly Pyrrhonian to refuse to believe him.[1]

The second instalment of yet a year later does not make good the half-promise to examine Book II but proceeds directly to Book III, " Of Morals." It starts off in a somewhat conciliatory fashion :

> [The author] here treats of matters the most interesting to the welfare of man and, these matters being designed to be understood by everybody, he has attempted to express himself in a manner more intelligible than he had used in the preceding volumes. . . . We hope, with all our heart, that the public will judge of these things as the author wishes; but we are not without fear that the *ordinary reader* will still complain that his metaphysics is a little obscure and that he should have thrown more light upon it. Sometimes it is unfortunate to have too much talent and penetration. . . . [However], we feel obliged to do him justice so far as he has really made some effort to adapt himself to the capacity of a greater number of people. Not only has he taken more care on occasion to clarify his principles by popular examples, but he has as well frequently added notes to the text ; and these notes, at least the greater part, truly shed some light.[2]

Having made this gesture, the reviewer shortly returns to his more normal position, for, after sketching Hume's system, he writes :

> Such is the general plan of morality that our author has formed. It is, in our opinion, rather an outline of principles of the art of good living than a system complete and connected in its parts. He could have more order in it, more clarity, more detail ; but also he could not have more paradoxes in it, more singular associations of ideas and of words which no one had yet taken it into his head to bring together ; more passages calculated to excite the curiosity of people who do not like the beaten path, and, to sum up, more new and original thoughts. We must make allowances to the subtle and ingenious anonymous author for these things. *Non omnis fert omnia tellus.*[3]

At the outset the reviewer finds himself nonplussed by Hume's discussion of justice and injustice : " I own my ignorance ingenuously. I have been stopped at the beginning of the first

[1] *Bibliothèque raisonée*, 24[2] (1740), 354-55. [2] *Idem.*, 26[2] (1741), 411-14.
[3] *Idem.*, 26[2] (1741), 415.

Part." But he bravely proceeds to explain how Hume founds the principles of morality on sentiment:

> Such is the whole system of our author. When Mr. Hutcheson proposed it in his *Enquiry Concerning the Original of our Ideas of Virtue and Moral Good*, competent people [i.e., Gilbert Burnet and George Berkeley] found three major faults with it. . . . I do not know how it happens that our author has not found it appropriate to examine the objections of these authorities. They would have opened up a fine field for his speculations and his profound metaphysics. Perhaps it is modesty, perhaps also, prudence.[1]

If Hume is pure Hutcheson in regard to sentiment, he is also, according to the reviewer, pure Hobbes in regard to the origin of justice and of property rights. "Here, as you can see, is the system of Hobbes dressed up in a new taste. If that philosopher had produced it in this fashion I doubt if he would have had such a reception in the world." Yet the reviewer closes in a more friendly manner, deprecating his ability to follow the thoughts of the author and to render them intelligible to his readers. "The surer plan is to send directly to the work those readers curious for subtleties and for metaphysical abstractions. If the author wished to add a glossary to it, he would save them much work."[2]

(c) *Nouvelle bibliothèque.* The *Nouvelle bibliothèque, ou histoire littéraire des principaux écrits qui se publient*, published at The Hague, included on its editorial staff, J. Barbeyrac and A. B. de la Chapelle, as also the Marquis d'Argens. The *Treatise of Human Nature* is given a preliminary notice followed by a review of forty-six pages. The notice of the first two volumes appears in the "Literary News from London" in the October number for 1739.[3] The comment is interesting: "Although the ideas of the author approach closely in some places to those of Dr. *Hutcheson* on the moral sentiment and on the human passions, there are yet many original things in this new treatise, which, however, is only the commencement of a more extended and more complete work." The two sections of the review proper, in the July and September issues of 1740, cover only the first volume of the *Treatise*.[4]

The reviewer for the *Nouvelle bibliothèque* maintains a factual, serious, and impersonal tone throughout. At the outset he emphasizes the difficulties of metaphysical speculation in general and then proceeds to the *Treatise*. "So we ought not to be surprised if the reasonings and the principles that the author

[1] *Idem.*, 26² (1741), 415, 423-24.
[2] *Idem.*, 26² (1741), 427.
[3] *Nouvelle bibliothèque*, 4 (1739), 302.
[4] *Idem.*, 6 (1740), 291-316; 7 (1740), 44-63.

proposes to develop are difficult to comprehend. We even advise those who do not like philosophical discussions to skip this article entirely."[1] His critical comments are few in number yet his personal reactions manage to emerge. Hume's development of the distinction between impressions and ideas he finds contradictory; Hume's analysis of the vacuum, he feels, will "satisfy neither the Cartesians nor the Newtonians"; finally, finding himself at the end of the allotted space for the first article when in the midst of the discussion of cause-and-effect, he remarks, "The matters which remain for us to run over are too curious to pass by lightly and too abstract to explain to-day; it will be better for us to come back to them another time."[2]

And so, in the September, 1740, issue, he returns to Hume's theory of belief as a vivacious idea, commenting: "It seems to us that he has confused the evidence with an act which immediately follows it, I mean, with judgment." A certain, almost begrudging, admiration of the author of the *Treatise* is admitted: "Allow me to remark in passing, that if the principles of the author on impression, belief, and custom are not solid, it must at least be admitted that he uses them skilfully to clarify several intellectual phenomena equally important and difficult to explain." And the review ends with a bare description of the contents of Volume II, "Of the Passions," leaving it up to "those who like these matters, treated in the abstract . . . to read them in the original. . . ."[3]

II. Comments in German.

If Hume had a friend among the critics associated with the French-language press in Holland, he clearly had none with the German. For two notices of the *Treatise* in German are alike hostile.

(a) *Neuer Zeitungen.* Under the heading of London, May 28, 1739, the *Neuer Zeitungen von gelehrten Sachen*, a Leipzig periodical, noticed Hume's *Treatise*. The notice[4] is short and may be given here in full:

> A new free-thinker has published an exhaustive *Treatise of Human Nature*, 2 volumes, octavo. In it he attempts to introduce the correct method of philosophy into moral matters, examining and explaining, first of all, the characteristics of the human understanding and then the effects. The author's intentions are sufficiently betrayed in the sub-title of the work, taken from Tacitus: *Rara temporum felicitas, ubi sentire, quae velis, & quae sentias, dicere, licet.*

[1] *Nouvelle bibliothèque*, 6 (1740), 292. [2] *Idem.*, 6 (1740), 295, 308, 316.
[3] *Idem.*, 7 (1740), 46, 55, 62-63. [4] *Neuer Zeitungen* (1739), 381.

(b) *Göttingische Zeitungen.* In a learned journal that has been in continuous existence from 1739 to the present times, the *Göttingische Zeitungen von gelehrten Sachen*, Hume's *Treatise* is reviewed in the 1740 number under a London, January 7, heading.[1] The three-page article opens: "In this book on human nature the anonymous author attempts to introduce the experimental method into moral philosophy. The book ranks so high among unusual literature that we cannot abstain from elucidating some of the author's singular thoughts in order to give an idea of the main subject to be dealt with later." Then, after a sketch of Hume's methodology, Book I is commented on, section by section:

> Thus all ideas originate in impressions which consequently correspond exactly to them. So the author hopes at one stroke to do away with the existence of innate or inherent ideas. His conceptions of abstract ideas, of the ideas of memory and imagination, of the association of ideas, of the concept of substance and modes are so abstract, not to say confused, that one can scarcely hope that he will shed more light on moral truths than has been done heretofore. The author agrees with Dr. Berkeley, whose theories he considers irrefutable in all respects, that all supposedly general ideas are but particular ones associated with certain words that give them a wider meaning so that, on occasion, they can convey other single related conceptions. If the reader will accept these beliefs as completely as their author does, then he will no doubt be convinced of their validity; yet we fear that this will not be the case should the reader not accept the evidence here presented. . . . His conceptions of certainty and probability are peculiar. He has a great talent for presenting in confused terms what others have stated clearly. The lucidity of the idea that all that comes into existence must have a cause is matched by the obscurity of the author's examination of it. The necessity of these causal concepts exists with him only in theory and not in the existing object itself, as all necessity of cause-and-effect consists only in the propensity of the mind always to think of two objects together. Oddly enough we shall not even think of the consequences the author derives therefrom! . . . This will suffice to give an idea in anticipation of the other volume.

Oddly enough, too, the reviewer never fulfils his promise, twice made, of carrying on with the remainder of the *Treatise*.

III. Conclusion.

At the time of publication, Hume's *Treatise of Human Nature* was by no means completely ignored either in Britain or on the Continent. And let it be emphasized that there may be other comment that I have not yet been able to find, some of which perhaps in the course of time will be forthcoming. In that

[1] *Göttingische Zeitungen* (1740), 9-12.

regard, it might be useful to record here that the *Treatise* was not reviewed in some journals which might conceivably be thought likely sources. Among these are the *Acta Eruditorum*, *Journal des savants*, and *Mercure de France*. Other periodicals yielding a negative result are *Le pour et contre*, *Mémoires de trévoux*, *Mémoires pour servir à l'histoire des sciences et des arts*, *Observations sur les écrits modernes*.

Was Hume acquainted with the Continental reviews? On the basis of our knowledge of his character it is certainly to be expected that he would be alert to reactions of the learned journals. After all, just what was he awaiting during those anxious months following publication? Good sales, yes, but good reviews, even more so. For better or worse, he had placed himself before the learned world; and reviews are the chief means of ascertaining their approval or disapproval or, what is worse from the point of view of the author, their indifference. It may legitimately be assumed, therefore, that Hume attempted to keep up with the reviews. It is certain, of course, that he knew of the article in the *History of the Works of the Learned*; and it seems clear enough from his relationship with Desmaizeaux that he was aware of the articles in the *Bibliothèque raisonnée* and the *Bibliothèque britannique* as well. Of the others, there is no evidence, direct or indirect; his acquaintance with the German is somewhat questionable, although he would undoubtedly have been interested had they been brought to his attention. From February, 1739, to March, 1740, only nine of his letters have survived and none at all between March, 1740, and June, 1742. Should any substantial group of letters of the three-year period following the publication of the *Treatise* ever come to light, there is every likelihood that evidence would become available of Hume's awareness of some others of the Continental reviews.

Pending such new discoveries, it is safe to assume that the despondent young author found little in the Continental reviews to cheer him up. What specifically did he find? He found that the critics displayed little understanding of, and no sympathy for, his proposed intellectual revolution. Their minds were totally unprepared for a Newton of moral philosophy, as Hume sanguinely regarded himself. They were, moreover, timorous of the implications of his system to religion and to practical morality. Hume found that his youthful ebullience over his great discoveries was interpreted as dogmatic and egotistical; and he must have been dismayed to find that he was almost universally charged with being abstract to the point of unintelligibility.

It is this disappointment, unquestionably, that is reflected in the famous statement that the *Treatise* "*fell dead-born from the Press.*" It now seems evident, in view of the new information presented in this article, that Hume did not mean that the *Treatise* passed totally unnoticed, but that it did not have the reactions in *philosophical* circles that he had hoped for and that it merited intrinsically. There is plenty of other evidence that the *Treatise* was more widely known than has generally been supposed; but this is not the place to discuss what is essentially of biographical rather than of philosophical significance.[1]

What may be said, on the other side of the question, about the introduction to the *Treatise* afforded by the Continental reviews to philosophical readers? Regarded as a whole, the Continental reviews presented Hume as a critic of Locke and a follower of Berkeley and Hutcheson. They emphasized, on the one hand, his critique of the necessary connexion between cause and effect and, on the other, his naturalistic development of sentiment. His attack on the empirical basis of geometry is alluded to without being fully analysed. And, in general, his scepticism is stressed at the expense of his constructive system. Finally, the name of the author was not unavailable.

The possibility of an early diffusion and influence of Hume's thought is thus opened up. The learned journals were more widely read than their relatively small circulation might seem to indicate, as librarians customarily consulted them for advice on the purchase of books. It is, therefore, conceivable that later research may bring to light some indication of such a diffusion and influence of the *Treatise* itself in the decade before the publication of the two *Enquiries*. But that is a problem lying beyond the scope of the present article.

[1] This new information will be presented in my forthcoming biography of Hume.

III.—THE NERVOUS SYSTEM AS PHYSICAL MACHINE: WITH SPECIAL REFERENCE TO THE ORIGIN OF ADAPTIVE BEHAVIOUR.

By W. R. Ashby.

§ 1. Introduction.
§ 2. The problem.
§ 3. Machines.
§ 4. Breaks.
§ 5. Equilibrium.
§ 6. Machines in time.
§ 7. Applications to living organisms.
Summary.

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§ 1. *Introduction.*

A century's study of the nervous system has shown that one of its main functions is that it brings the organism into adaptation with the environment. The adaptation is of two types: (1) There are fixed, inborn mechanisms, the reflexes, which are necessarily adapted by natural selection. But with these we are not concerned in this paper. (2) In addition to the reflexes, however, there are formed in each organism during its life a large number of adaptive reactions which are not inborn but which have to be developed in each individual according to the environment. The fact that "a burned child dreads the fire" exemplifies this second type. There is no need to stress its wide occurrence nor its importance.

The author has been concerned for some years with attempting to reconcile the fact of the occurrence of this type of adaptation with the usual hypothesis that the brain is a machine, *i.e.*, a physico-chemical system. In the opinion of some, there is a fundamental impossibility that any isolated machine could show behaviour of this type; but the impossibility has not been clearly proved. It seemed possible, however, that a more elaborate study of the essentials of "machines" might show possibilities hitherto overlooked.

Working in this direction the author was led to examine in minute detail, *i.e.*, mathematically, exactly what is meant by a "machine", and later to study the peculiar events which occur when a machine "breaks". Having discovered and established a mathematical and rigorous theory of the subject, he found that one of its first applications was to give a straightforward account of the *physical* origin of adaptive behaviour.

The paper is divided into three parts: The first (§§ 1 and 2) clarifies the problem and gets it clearly stated. The second part (§§ 3-6) is concerned with the fundamental properties of machines. In the third part (§ 7) is given the application to living organisms, showing how the theorems given in the second part solve the problem.

§ 2. *The problem.*

Before we can proceed we must change the form of the problem. Physics, and mathematics, know nothing of "adaptation" as such; so we must first convert our biological problem into its equivalent physical form. We therefore re-examine it from the physical point of view so that we know what *physical* features to look for.

With regard to the nervous system, it is treated here as a purely physico-chemical system, of dynamic type. We use the concepts that parts of it (the "receptors") are affected by the environment, that actions and interactions occur in the nervous system, and that some parts (the "effectors") act on the environment.

What of the "adaptive behaviour"? The meaning of "behaviour" is clear enough, but what of "adaptive"? When we examine an adaptive reaction, we find certain striking features, features which any physical theory is bound to explain:

(1) The development of the reaction involves, presumably, the development in the nervous system of some special arrangement or *organisation* (in a general sense) to mediate it, no single neuron being able to produce the result.
(2) This organisation is of neurons and has the remarkable property that the neurons develop the *right* organisation and not one of the almost infinite number of possible wrong ones.
(3) The neuronic activities in this organisation are observed to be *co-ordinated* so that they form a coherent whole.

Our problem may be provisionally stated by saying that we wish to find whether any machine-like system can produce these results—or if not we should like to *prove* it impossible.

But so far we have given no physical meaning to the "right" result, nor to "adapted" behaviour. It will now be suggested that an organism which is "adapted" to its environment is equivalent to a machine in a state of equilibrium. (We must be careful here to distinguish between (1) the state where the animal is already adapted to the environment and (2) the process by which that final state is achieved. The word "adaptation" is

often used to cover both. In this section we shall discuss the characteristics of an animal which has already reached that state. The question of how it was arrived at will be the subject of the subsequent sections).

First, an animal "adapted" to its environment is, above all, self-preserving. This means, on closer examination, that it does not starve, nor die of thirst, nor injure itself, nor allow itself to be deprived of oxygen, and so on. More precisely, this means that there are certain variables such as the amount of sugar in the blood, of water in the tissues, of mechanical strains on bone and sinew, of oxygen supply to the tissues, and so on, and that an adapted animal's behaviour always ensures the maintenance of these variables at certain more or less fixed values. Thus, typical adapted behaviour would be shown by a fish which sought out food when it was hungry, or by an animal whose respiration became brisker if placed in an atmosphere deficient in oxygen. In all such cases the animal shows its adaptation by producing behaviour whose end result is to bring back these important variables after a disturbance. Pavlov (1927) states the matter with perfect clarity:

> "The animal must respond to changes in the environment in such a manner that its responsive activity is directed towards the preservation of its existence. . . . Being a definite circumscribed material system, it can only continue to exist so long as it is in continuous equilibrium with the forces external to it."

Is a state of equilibrium *sufficient* to ensure adaptation? It is doubtful whether any other characteristic may be imposed without arbitrariness. Thus, suppose an animal does always produce behaviour which keeps its food supply constant, its water intake constant, its oxygen supply adequate, its carbon dioxide level in the blood correct, etc., have we any right to say that the behaviour, however peculiar it may seem to us, is not adapted? When we consider the extraordinary range of types of living creatures and the great variety of ways in which they maintain themselves, it seems that, in the simpler animals at least, any active equilibrium which maintains steady the essential physiological variables must be called "adapted".

Let us now reverse the argument, by examining a machine in equilibrium and observing to what extent it imitates adaptive behaviour. As a simple example we may take a pendulum hanging vertically. If we push the bob away from the lowest point to the right, we find that forces are set up tending to move the

bob to the left. And if we push it to the left it immediately pushes to the right. And the behaviour is similar if we push or pull it to or from us. Again, consider a thermostat, as an interacting series of parts which keeps the temperature of a water bath constant. If we upset the thermostat by deliberately chilling the water, we find that changes occur in the system, the net result of which is that the water is heated more strongly. *It is an elementary property of all systems in equilibrium that they react so as to oppose disturbance.*

Space prohibits an exhaustive discussion but it may therefore be reasonably assumed (Ashby, 1940) that " adapted " behaviour is equivalent to " the behaviour of a system in equilibrium ". If this hypothesis be conceded, the problem takes the new and more definite form : *how does the nervous system develop such an organisation internally that, as a machine, it shall be in equilibrium with its environment ?*

But before this can be answered we must know more about " machine ", " organisation " and " equilibrium ". (The reader, however, is recommended to proceed direct to § 6, referring back only if the argument seems unconvincing or faulty.)

§ 3. *Machines.*

(The next four sections are required to establish the conclusions of § 7. But to proceed by stating the conclusion and then proving it would mean that an extensive mathematical system would have to be proved bit by bit in inverted order. I have preferred to develop the theory in its natural direction. When this has proceeded far enough I can then use this to tackle the original problem. The theory can be, and is being, developed in many directions. Here it will be given only so far as is required for the subject of this paper).

The words " machine " and " organisation " have a multitude of possible meanings, but here we are concerned with the idea of a number of parts which interact on one another, *e.g.*, neurons which transmit impulses to one another. Roughly, the set of parts is the machine, and the way they are put together is the organisation. (Note that we are in no way restricted to systems with Newtonian dynamics).

A typical example of a dynamic organisation is given by a frame with a number of heavy beads in it, the beads being joined together by elastic strands to form an irregular network. Such a network, if pressed out of position and then released, will move (as specified by the positions and velocities of the beads) with

a pattern of movement which depends (1) on the original displacement and (2) on the constant factors such as the masses of the beads, the lengths and arrangement of the strands, the shape of the frame, etc. It is an organisation because the movement of any one bead at a given instant depends on where the other beads are at that instant, *i.e.*, it is impossible to predict the movement of one bead merely by studying that one bead.

To handle such a case we start by labelling the necessary variables $x_1, x_2, \ldots, x_n$. It is immediately necessary to appreciate that the number of variables is not the same as the number of material bodies present. Thus, if the frame above contained three beads, the number of variables would be eighteen : three co-ordinates of position and three of momentum for each bead.

A " configuration " of such a system (at a given instant of time) is the set of numerical values of the variables. The " behaviour " of such a system is defined by the successive configurations with the time-intervals between them.

It is now postulated that our system, whatever it is, must be such that each configuration determines the configuration which follows it. (This hypothesis lies at the root of most dynamic science and means that we are dealing with a determinate, and not a capricious, system). It is a *fundamental theorem* that, for a system to have this property, it is necessary and sufficient for it to be specified by equations of type :

$$\left.\begin{aligned} \frac{dx_1}{dt} &= f_1(x_1, x_2, \ldots, x_n) \\ \frac{dx_2}{dt} &= f_2(x_1, x_2, \ldots, x_n) \\ &\cdots \\ \frac{dx_n}{dt} &= f_n(x_1, x_2, \ldots, x_n) \end{aligned}\right\} \quad . \quad . \quad (1)$$

Such a system is a *complete* system. It is specially to be noted that t and functions of t other than $x_1, x_2, \ldots, x_n$ are absent explicitly from the right-hand side. The f's must be single-valued, but no restrictions of continuity or differentiability are implied. A " machine " will now be used exclusively to mean a complete system.

As an example we may consider a pendulum swinging sideways through a finite angle x_1. To make the system complete we also need the angular velocity (x_2). The equations (in our form) are :

$$\left.\begin{aligned}\frac{dx_1}{dt} &= x_2\\ \frac{dx_2}{dt} &= -\frac{g}{l}\sin x_1\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (2)$$

where g is the acceleratio n of gravity and l the length.

The set of equations (1) has a unique set of solutions of form

$$x_i = \mathrm{F}_i(x_1^0, x_2^0, \ldots, x_n^0;\ t)\ (i = 1, 2, \ldots, n) \qquad (3)$$

where

$$\mathrm{F}_i(x_1^0, x_2^0, \ldots, x_n^0;\ 0) = x_2^0\ (i = 1, 2, \ldots, n)$$

and $x_1^0, x_2^0, \ldots, x_n^0$ is an arbitrary set of constants. These equations (3) give the observed behaviour of the system after it has been started at $t = 0$, at the configuration $x_1^0, x_2^0, \ldots, x_n^0$. In a complete system the functions F_i define a *finite continuous group*. That such a set of equations as (1) has all the ordinary properties of machines may be shown in ways too many to give here.

Following the methods usual in statistical mechanics, each configuration $x_1, x_2, \ldots, x_n$ defines a "representative point" in a geometric n-dimensional space with $x_1, x_2, \ldots, x_n$ as coordinates. It is a well-known theorem that equations (1) define a field of paths in this space such that if we start at a given configuration and follow the behaviour defined by (1), then the representative point will follow the path it was started on. That the field should be constant in time, it is necessary and sufficient that the system be complete. This is important since it means that only complete systems can be said to have any fixity of behaviour.

It should be noted that:

(1) Each *point* in the x-space corresponds to a single instantaneous state of the machine.

(2) Each *path* corresponds to the behaviour which follows the starting of the machine from some particular configuration.

(3) The *field* corresponds to the equations (1), and gives all the possible behaviours of that particular machine.

(These facts are illustrated by the example of the next section).

The variables are, as we said, those items which alter with the time. But behind these are the "constants", or parameters, of the system. (The two must be kept most carefully distinct in the reader's mind). If we alter a parameter the effect on equations (1) is to change the f's; and its effect on the machine is to alter the way one variable affects another. We therefore define the "organisation" of the system as corresponding to, and

specified by, the f's of equations (1). Prolonged testing has convinced me that this identification is valid and fruitful. A set of equations

$$\frac{dx_i}{dt} = g_i(x_1, \ldots, x_n ; p_1, \ldots, p_r) \; (i = 1, \ldots, n) \quad (4)$$

will therefore have as many organisations as the set $p_1, p_2, \ldots, p_r$ has values.

§ 4. *Breaks.*

Having defined " machine " and " organisation ", we now proceed to examine exactly what is meant by a machine " breaking ". To fix the ideas, suppose we have a pendulum swinging from side to side and that the string breaks so that the bob falls downwards. The first point to notice is that after the break, the result, no matter how chaotic, is still a machine, within our definition, except that it is now a different machine. Examination of other cases soon shows the first point : a break is a change of organisation.

The next point to consider is whether the changes which occur at the moment of breaking are ordinary, *i.e.*, representable by the equations of a complete system or not. It is clear that there is no question here of anything going against the fundamental laws of nature. All breaking events, whatever they are, are perfectly natural : *e.g.*, the breaking of a cord, the fusing of a wire, the snapping of a rod. *Therefore they are deducible from their immediately preceding states.* And this means that the events of the break occur within a complete system. It means that when we have a machine (like the pendulum above) and then a break, and then a second machine (the pendulum falling downwards) we can, if we like, set up more comprehensive equations which will treat the whole set of events as the ordinary events of one single machine.

This fact, that we may include a break-event in the equations of a complete system, is so important that I must give an example. It will answer much possible criticism and will illuminate the whole paper. (It has been selected to be as short as possible).

Consider a weight of mass m hanging downwards under gravity on a (massless) elastic strand. If the elastic is stretched too far it will break and the mass will fall. This provides, I think, a perfectly natural and typical example of a break.

Let the elastic pull be k dynes for each centimeter stretch from its position of rest (and assume for simplicity that the elastic

expands similarly when compressed). Measure x, the position of the mass, downwards from the position of the elastic when there is no mass. The movement of the mass (vertical only) is usually written

$$\frac{d}{dt}\left(m\frac{dx}{dt}\right) = gm - kx,$$

where g is the acceleration due to gravity. This is not in our standard form but becomes so on writing $x_1 = x$, $x_2 = dx/dt$, giving

$$\left.\begin{aligned} \frac{dx_1}{dt} &= x_2 \\ \frac{dx_2}{dt} &= g - \frac{k}{m}x_1 \end{aligned}\right\}$$

If the elastic breaks, k becomes 0 and our equation becomes

$$\left.\begin{aligned} \frac{dx_1}{dt} &= x_2 \\ \frac{dx_2}{dt} &= g \end{aligned}\right\}$$

illustrating the change of organisation.

To bring in the break explicitly, suppose the elastic will stretch to a point X and then snaps. k now becomes a variable with two values, K and 0, say, whose development in time proceeds by the substitution

$$k' = \lambda(x_1, k)$$

(not written dk/dt since "dk" is finite) where k' is the value of k consequent on its having had the value k an instant before, and where λ is a functional symbol defined by

$\lambda(x_1, k)$		k	
		0	K
x_1	$< X$	0	K
	$> X$	0	0

Examining this substitution we find, assuming x_1 starts at less than X and that k starts at K, that as long as x_1 remains less than X, k will keep the value K (*i.e.*, remain unbroken). But if x_1 once becomes larger than X, then k will go to 0 and will stay there, whatever happens subsequently. It thus describes correctly what happens to the elasticity k. So the final set of equations is

$$\left.\begin{aligned} \frac{dx_1}{dt} &= x_2 \\ \frac{dx_2}{dt} &= g - \frac{k}{m}x_1 \\ k' &= \lambda(x_1, k) \end{aligned}\right\}$$

It is obviously a complete system. The set of equations, though a little unusual in form, *does define the solutions*, thereby demonstrating the possibility of a break being specified correctly in a complete system even when the break occurs in the middle of the time covered. (In actual practice one would use some continuous approximation to the k'-substitution).

A break can therefore be considered to be an ordinary incident in the activity of one (more comprehensive) machine.

We now ask the converse question: Under what conditions is it possible to divide the activities of a machine into two parts (in time) so that we can say that there was a machine which broke and formed the second? (We ignore the trivial case of just changing its name).

It may be shown that there is only one possible way: among the variables there must be one which stays constant for a time and then changes suddenly to a new value, also constant. In other words, one of the variables must be a step-function of the time. Thus, suppose it to be x_n, that it can take only the two values x_n^0 and x_n^1, and that it starts at x_n^0. When this is so, then as long as that particular variable stays constant we may, if we like, no longer write it explicitly, lessen the variables by one and simplify the equations

$$\frac{dx_i}{dt} = f_i(x_1, \ldots, x_{n-1}, x_n^0) \quad (i = 1, \ldots, n-1)$$

to $$\frac{dx_1}{dt} = g_i(x_1, \ldots, x_{n-1}) \quad (i = 1, \ldots, n-1)$$

where

$$g_i(x_1, \ldots, x_{n-1}) \equiv f_i(x_1, \ldots, x_{n-1}, x_n^0) \; (i = 1, \ldots, n-1).$$

And the g-equations will remain valid as long as x_n maintains its first value of x_n^0. When, however, we arrive at the time when it suddenly changes, the g's no longer represent the machine correctly, and we shall have

$$\frac{dx_i}{dt} = f_i(x_1 \ldots, x_{n-1}, x^1{}_n) \quad (i = 1, \ldots, n-1)$$

which will simplify to, say,

$$\frac{dx_i}{dt} = h_i(x_1, \ldots, x_{n-1}) \quad (i = 1, \ldots, n-1)$$

for the second part of the machine's activities. We may now, if we like, say that the first machine (specified by the g's) has broken and become the second (specified by the h's). This fact, that *a step-function in a complete system creates two new complete systems*, is the mathematical point of the whole paper.

(We must be careful to note the possibility of confusion over "*the* machine". If we are referring to the system which does not break, then "the machine" refers to the variables $x_1, x_2, \ldots, x_n$, but if we are using the "break" concept, then "the machine" refers to the system of $n - 1$ variables $x_1, x_2, \ldots, x_{n-1}$. The distinction is sharp. If there are large numbers of breaks possible, the distinction becomes very important as the two "machines" will usually be profoundly different in properties).

There is therefore complete correspondence between the properties of breaks and those of step-functions, and the final definition of a break is as follows: If, in a dynamic organisation (as defined above), one of the variables is a step-function of the time, then at the moment when it changes value we may, if we like, say that the machine composed of the other variables "broke" at that moment and formed a new machine.

We may now examine in more detail the conditions under which the break will occur. If v is a continuous function of the x's, the surface

$$v(x_1, \ldots, x_n) = 0$$

will divide up the x-space into regions so that on one side of the surface the step-function has one value and on the other side of the surface the other value. Now examine v in the $(n - 1)$-dimensional space of $x_1, x_2, \ldots, x_{n-1}$, *i.e.*, the space of the machine which breaks. There will be two surfaces possible:

$$v(x_1, \ldots, x_{n-1}, x_n^0) = 0$$

and

$$v(x_1, \ldots, x_{n-1}, x_n^1) = 0$$

only one of which can exist at a time. Such a surface is called a "break-surface".

The importance of a break-surface is that it specifies the conditions under which x_n will break, *i.e.*, change value. Thus, suppose the machine starts with x_n at x_n^0, then as long as the representative point does not cross that surface, so long will x_n remain at x_n^0, *i.e.*, so long will the machine remain unbroken. Should the point cross the surface, then x_n will change to x_n^1 and the machine will break. (What happens after the break depends on the position of the other break-surface. If, as often happens, it is entirely at infinity, then the break is irreversible).

§ 5. *Equilibrium.*

The usual elementary definitions of equilibrium (*e.g.*, "that the vector sum of the forces is zero") are of no use here for we are considering more general systems, and such a definition could not be applied even to a thermostat, for instance, though the latter exhibits obvious equilibrium. (The concepts of "neutral" and "unstable" equilibrium are of little interest here and will not be considered). More general definitions seem to be lacking, though the essential idea is clearly stated by Lorentz (1927): "By a state of equilibrium of a system we mean a state in which it can persist permanently".

Ordinary equilibrium is an example of the physicist's concept of a "steady state", of which the essential property is that, from some configuration onwards, the variables concerned are always bounded. But this general definition makes possible all sorts of curious special cases, physically rare but mathematically possible. In practice, "equilibrium" usually refers to a sub-case, defined here as occurring when a part of the field in the x-space is filled with paths all of which converge to and terminate at one point. It may be shown that, under the conditions discussed later in this paper, it is more probable than other steady states.

Equilibrium has some special properties which we must notice. Firstly, since the paths lead inward, the system tends to the configuration represented by the central point; so if it is disturbed slightly from this it will automatically develop internal actions or tendencies bringing it back to that state. In other words, it appears to oppose any disturbance from that state. Further, if we disturb it in different ways (*i.e.*, displace the representative point in different directions) it will develop different tendencies with different disturbances, the tendencies being always adjusted to the disturbance so as to oppose it. Clearly this is characteristic of any system in equilibrium whether living or dead.

§ 6. *Machines in Time.*

We have now completed the necessary framework and can handle with quantitative accuracy questions relating to machines, equilibria, and breaks. We proceed to develop a fundamental principle by asking the simple question: "What happens to dynamic organisations in time?"

The usual answer is that the system moves for ever according to the equations governing it. But this answer is no longer satisfactory when we use the concept of a "break". The idea

of variables increasing indefinitely far is a mathematician's extrapolation, and it does not correspond with what happens in real dynamic systems; not, at least, in the type of system we are considering here. Thus, quicker movements in a machine lead, if fast enough, to mechanical breaks; increasing electric currents or potentials lead to the fusing of wires or the breakdown of insulation; increasing pressures lead to bursts; increasing temperatures lead to the apparatus itself melting; even in chemical dynamics, increasing concentrations lead to saturation complications or changes of phase (a break as defined above). It is therefore a common observation that machines break if their variables go far enough.

Under these conditions every machine must end at some equilibrium. Thus, suppose we start with a great number of haphazardly assembled machines which are given random configurations and then started. Those which are tending towards equilibria will arrive at them and will then *stop there.* But what of the others, some of whose variables are increasing indefinitely? In practice the result is invariable—something breaks. After a break the organisation is changed, and therefore so are the equilibria. This gives the machine fresh chances of moving to the new equilibrium or, if not, of breaking again. It is as if a gambling game were being played with the curious rule of

Heads—I win; Tails—we toss again!

Clearly there is only one practical ending. This may be stated in the approximate but arresting form: dynamic systems stop breaking when, and only when, they reach a state of equilibrium. And since a break is a change of organisation the principle may be restated in the equivalent form: if we allow breaks to occur, then all dynamic systems change their organisations until they arrive at a state of equilibrium.

It is important to note that up to this point we have made no special hypothesis at all. The principles described are common and necessary to all machines whatever.

We now examine the behaviour of a machine with a large number of breaks available, the conditions of break, *i.e.*, the break-surfaces, being all approximately the same. (This will tend to occur if the machine is built up of great numbers of small parts which are all approximately similar to one another). Under these conditions, the machine is bound to end at an equilibrium whose paths avoid the break-surfaces: the reason being that if the point follows a path meeting the surfaces, a break occurs,

and the old field is destroyed. Only a path avoiding the break-surfaces can survive. In this way *a layer of break-surfaces acts as a barrier to the representative point.*

Finally, suppose the break-surfaces surround a given region of the x-space completely. Start with the representative point within the region. We then follow it as before: If the point reaches an equilibrium within the given region, it stops there. If not, it will cause breaks until eventually a field arrives with an equilibrium within the region. Only when this arrives can the system stop breaking. It is clear, therefore, that a layer of break-surfaces round a given region will automatically alter the field until there is an equilibrium within the given region. Again we may re-state this in terms of organisation: *A layer of break-surfaces round a given region will automatically ensure the establishment of an organisation having an equilibrium within the given region.*

This is the point of the whole paper.

§ 7. *Application to Living Organisms.*

It has been shown in the previous section that machines with large numbers of breaks available have, in consequence, certain highly characteristic properties—properties not hitherto studied. To what extent may these properties be regarded as solving the problem we started with? As stated in § 2 it was: "How does the nervous system develop such an organisation internally that, as a machine it shall be in equilibrium with its environment, the equilibrium keeping all important variables within certain (physiological) limits?"

My answer is simple: let it, as a machine, break every time the variables go outside these limits. Then by the proofs of § 6 the nervous system is bound to develop the features required. But this needs amplification.

(*a*) Firstly, is the animal a "machine" in our sense? The animal by itself is incomplete, the environment being an essential part of the whole system. Without an environment, behaviour on the part of the animal would be both causeless and effectless. But when we have both animal and environment included, then the system is complete and the two form a machine in our sense. (It should be noted that we now make no essential distinction between the animal and its environment: both contribute to the organisation of the whole, both act dynamically on themselves and on the other, any equilibrium must stabilise both, and as physical systems the above principles must govern both).

In the nervous system the " variables " are mostly impulse-frequencies at various points in the nerve-network. And as the impulses at one point affect in various ways the impulse-frequencies at other points, the nervous system is a dynamic system par excellence.

(*b*) The next question is to ask what " breaks " there are available in the nervous system. A first reply is that there must be breaks present, since it may be proved that there is no other way in which an isolated machine can change its organisation.

And we must not overlook the fact that the precise physiological nature of the breaks is, for our purpose, of no importance. The properties we have proved are true for all breaks whatever, *i.e.*, for all physical variables which behave as step-functions.

But it does not seem improbable to suppose that neurons have breaks available. The delicacy of the finer dendrons and of the " pieds terminaux ", the richness of the cells in protein-lipoid membranes, some of which may be mono-molecular in thickness, and the presence of fibrils are all suggestive. More definitely, Speidel's (1942) observation that nerve endings can be amoeboid, confirming *in vivo* what was observed in tissue culture, provides for my theory an almost ideal mechanism to embody the principles.

(*c*) The necessity in the theory that the breaks should be consistent is satisfied with reasonable probability since the cells of the nervous system are not likely to be all entirely different from one another in regard to their basic physiological properties.

(*d*) The large number of breaks required by theory may be supplied by each neuron, but a feature contributing to the same end is that, as a high intensity of excitation in one cell is likely to be followed by a high excitation in other cells to which it is joined, high excitations in one cell may tend to cause breaks in other cells, thus having the effect, relatively, of increasing the number of breaks available. Another fact having the same effect is that comparatively few breaks will supply a great number of organisations.

(*e*) The " region " of § 6 is around that configuration in which all neurons are stimulated at an intensity below that at which breaks start to occur. This means that, according to theory, the nervous system must always keep changing its organisation until it reaches an equilibrium with all neurons (and particularly receptors) stimulated to less than some particular degree. What this degree is must be laid down by heredity : it will depend on

the primary physiological necessities of the animal. But it may usually be assumed that any activity which prevents the receptors from being excessively stimulated will protect the animal from immediate physical danger. (Though tasteless poisons soon demonstrate that the protection is not absolute).

(*f*) We may summarise this section by pointing out that each requirement of theory has been shown, in (*a*) to (*e*), to be paralleled by a known property of the nervous system.

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Finally, the author is well aware that this theory needs considerable elaboration and development in order to make it fit better the actual properties of living things. The theory is markedly lacking in some directions, some of which are in process of being corrected. But the theory seems to be complete, as far as it goes, in the elementary cases and this, though simple, provides at least a solid foundation for the future.

Summary.

An outstanding property of the higher nervous system is that, in contact with a new and previously unexperienced environment, it develops and produces behaviour which is adapted to that environment. It is usually assumed that the behaviour depends on the development of an inner, neuronic organisation. But considering that the neurons might as easily develop one of the almost infinite wrong organisations, the fact that the right one is developed calls for explanation. Why should a network of neurons produce adaptive instead of chaotic behaviour? Psychology cannot rest content until it has found a *physical* answer to this problem.

The author has approached this problem from the point of view that the brain is a physical machine, and that "adaptive" behaviour is that which maintains it in physical equilibrium with its environment. This basic theme has been studied with all possible generality and precision. This has necessitated the construction of a special mathematical technique, and this has led to a number of new theorems on equilibrium and organisation in generalised dynamic systems.

The problem of a self-equilibrating physical system can now be attacked with both rigor and generality. The paper then shows how these new methods provide an essentially simple and clear-cut solution to the problem.

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IV.—SECOND THOUGHTS ON CAUSATION, DUALISM, AND INTERACTION

By Howard D. Roelofs.

In this essay I shall present as clearly and as concisely as I can, certain conclusions about causation, dualism of substances, and interaction. These conclusions are the result of reflections, at times sustained and systematic, at times intermittent and perhaps unconscious, extending over a period of years. I shall indicate the primary data and the chief arguments supporting my conclusions, but I shall present neither in any detail. Two considerations have persuaded me to this procedure and each is of sufficient importance for my present purpose to warrant statement.

The first is that my primary data, although of two sorts, are thoroughly familiar to those who may reasonably be expected to read this essay. One sort of data to which I shall refer, is found in the daily, ordinary experience of every one of us. I mean no more and no less than our ordinary use of, and reliance upon, particular causal connexions; our daily encounters with bodies and with minds, our own included; and finally the *prima facie* response of mind to body, of body to mind, which is a stable feature of human life both individual and social. The other source of primary data is the chief arguments of Hume and Berkeley, in the one case against causation, in the other against matter. Certainly if we, as philosophers, have a working knowledge of any material, it is of this. It may be that Hume's arguments are more securely in our grasp than the standard pattern of daily life, for some of us are better readers than we are observers. But since we are ourselves living and active, at least I so assume, the actuality can make good any deficiency in memory and in previous observations.

The second consideration impelling me to omit detailed exposition arises from what I take to be the nature of reasoning. There is a tradition that in philosophy the appeal is to reason. I accept that tradition and endeavour to act in accordance with it. But reasoning is essentially individual. There can be mass suggestion, there are methods of group persuasion, but reasoning is carried on by each of us not only for himself but by himself. The principles of reasoning are indeed common to us all, and when we reason correctly our reasoning is the same; yet the particular reasonings each one of us actually does are nonetheless

individual; in fact, they are private. A simple, formal argument, such as a single syllogism, can be put in front of us by another, our attention directed to it, and we can then reason along with him. But when premises are many and varied, when reasoning is complex, not completely formalised, and more often indicative of a probable conclusion than a demonstrative, then to accompany the first presentation of a debatable thesis with detailed arguments is more likely to produce confusion than persuasion. That is the second reason why I shall restrict myself simply to indicating data, suggesting arguments, and stating conclusions. I do not aim at demonstrating anything in this essay, and I entertain little hope of persuading any except those already convinced. My purpose is much more modest. It is to direct attention, to re-awaken concern for supposedly settled situations, to occasion new reflections on familiar themes. In a word, I hope either to encourage or to provoke some of my readers to think again on causation, dualism, and interaction.

That which started me on these second thoughts may or may not be equally effective with my readers, but I can think of no better stimulus and I shall begin with that. It was the chance conjunction of a number of circumstances, not one particularly notable in itself. Here they are: (1) a re-reading of Berkeley and Hume resulting in an enhanced sense of the cogency of their reasoning; (2) a re-reading of Moore's *Refutation of Idealism* and a glance at a few sentences in the introduction to Sabine's *History of Political Theory* in which Sabine states his acceptance of Hume's destructive analysis, these two events resulting for me in the enhanced sense that Moore did not refute Berkeley and that Sabine outdid Hume in making his practice contrary to his professed convictions; (3) the insistence of a scientist of my acquaintance that, although constancy of correlation is an important criterion, what science seeks to discover is not that, but causal connexions; and (4) the realisation that I myself regarded physical bodies as certainly real and so did most of my acquaintances, philosophic and scientific.

For philosophers this is a pretty kettle of fish. Philosophy appeals to reason, Berkeley and Hume are outstanding among philosophers for valid reasoning, and we who call ourselves philosophers refute neither Berkeley nor Hume, yet hold contrary beliefs. In this there is something amiss. What is it? Our practice? The arguments of Berkeley and Hume? Our estimate of what precisely those arguments prove? At that point, with those questions, my second thoughts began.

My procedure in this paper will now consist not in a detailed narrative of the course of those second thoughts but, as was explained, in an indication of the data and arguments considered, and a statement of the conclusions reached. In the matter of causation the primary data are the facts of practice found alike in ordinary living and in science. I suggest that those facts exhibit and substantiate the following. As regards causality, we possess a working understanding of the causal relation as involving agency, transeunt production or determination. As regards particular causal connexions, we possess reliable knowledge of *some* of these. These two conclusions go together; the same facts illustrate and substantiate both. It is the *blow* of the hammer that *drives* in the nail; the *waves rock* the boat; the *increased* flow of gasoline to the carburettor *produces*, when vaporised, mixed with oxygen, introduced into the cylinder, compressed and ignited, the *increased* force which *moves* the car faster. Those relations are correctly termed causal for the reason that in them there is transeunt control; and in those specific cases we know what is the cause and what the effect.

Having stated this much it is important that I make precise and explicit what is not claimed. No account is given of the *origin* of our knowledge of what a cause is. No explanation is offered of *how* agency operates. The generality claimed is in each case specific and limited: given, and within the complex of, waves, boat, and rocking, it is then said that the waves rock the boat.

What about Hume's arguments? They still stand. But care is needed in stating what they prove. He demonstrates for us, as for himself, that the principle of causality in its universal form, every event has a cause, has neither *a priori* nor *a posteriori* proof; he still proves that, antecedent to experience, we have no knowledge of what specific effect will result from any given cause; his exhibition is still conclusive that in all alleged cases of causal connexion between events external to ourselves, the nexus itself, even if present, is not the object of a simple perception as colours and sounds are. Further, Hume's analysis of voluntary activity as a source of our knowledge of causation is as inconclusive now as it was when he made it; and his own account of that origin in terms of habit and expectancy, is as mistaken. But what Hume proves does not touch what has been stated as to the knowledge of causation possessed and used by all of us. For to say that a genuine cause involves agency and that there are such, is not to say that *every* event has a cause; and to assert that the

waves rock the boat is not to claim that this was known antecedent to the empirical evidence on which the knowledge is based. In general, all that I have said of normal practice is coherent with and involves no contradiction of Hume's genuine demonstrations.[1]

Putting together the two types of evidence thus far presented, I find certain further statements are warranted. Hume's proof that the causal nexus is not the object of a simple perception, the debatable cases of voluntary actions being excluded, and our actual knowledge of particular cases of the causal nexus, provide, when taken together, the basis for the conclusion that this knowledge is an interpretation of perceived data. We perceive certain things, and by an interpretation we perceive also that there is a causal connexion of this with that. I hope that this use of the word 'interpretation' will not itself be misinterpreted. I need a generic term to cover without prejudice cognitions ranging from intuitions or spontaneous inferences to perceptions guided and developed by innate ideas. I say 'without prejudice' because in this essay I am not concerned with the details of *how* we know there is a causal nexus, but with the fact that we do know what such a nexus is, and with certain very important aspects of that knowledge. These are: as Hume proved, the nexus is not the simple object of a simple perception; as experience proves, our knowledge arises in perceptual activity. Therefore I call it an interpretation. Further, the genuineness of that knowledge is not to be confused with the validity of specific cognitions of *this* cause of *that* effect. We can and do make mistakes in particular cases; *perhaps* any instance selected for examination may be open to question; but even if that be granted, it does *not prove* either that *all* particular instances are mistaken, or that our knowledge of the nature of the causal nexus is illusory. It may also be true that the knowledge we have of causation is not complete. Even so, that does not prove that the partial knowledge we do have is spurious.

[1] At the present time it is fair to assume at least this much agreement about Hume: (*a*) His contention that, as regards causation, we perceive only a sequence of impressions, is an incorrect statement of the facts. (*b*) When such sequences are observed, they are not accepted as examples of causal connexion—*cf.* Kant's second analogy and the standard fallacy, *post hoc, ergo propter hoc.* (*c*) When perceptions of causal connexions are made, the epistemic claim is that the connexion is not between 'impressions' but between things or events. If to this last it is objected that it begs the question by assuming that we perceive objects, the proper reply is that this objection is not against the perception of causal connexions, but against the perception of objects in general. But in this essay, the existence of an external world and our knowledge of it are not in general under discussion.

To say that something is an interpretation is neither to eliminate it nor to establish that it is false. But it does follow that our cognitions of causal connexions, being interpretations, are inseparable both as regards significance and truth or falsehood, from the specific situations which they interpret. The general statement, 'one event causes another', has little significance, and its truth in strictness cannot be determined until it is given a specification such as 'hammer blows cause nails to penetrate wood'. That is the first result of these second thoughts: judgments of causal connexions are interpretations of empirical data, and their significance and truth are tied to the situations they interpret.

A complementary conclusion, negative in form, is obtained by considering together our knowledge of specific causal connexions and Hume's demonstration that no study of an alleged cause can disclose, prior to its performance, what its effect will be. From this I draw the conclusion that no restrictions and no specifications based simply on our knowledge of the nature or character of the factors being considered, are ever legitimate as regards possible causal relations. Given that X is a leopard and Y is a leprechaun, nothing can be said about possible causal relations between X and Y simply on the basis of our knowledge of what a leopard is and what a leprechaun is. It is possible that the sight of a leprechaun makes a leopard change his spots. I know of no empirical evidence on this matter. Lacking such evidence, it is idle to argue the matter one way or the other on the ground that the kind of thing the leopard is and the kind of thing the leprechaun is, make this or that causal relation possible or impossible.

This negative conclusion, and the earlier positive one that judgments of causal connexion are inseparable from the specific situations they interpret, do not rule out inductive generalisations about causes and effects. What is established is that no inductive generalisation about a particular type of causation can itself be generalised into a universal rule or metaphysical principle about causality *überhaupt*. Every inductive generalisation stating a causal rule is limited in its application to the type of situation out of which it has been derived and for which it is the generalisation. Within that restriction, the reliability of the inductions is a function of the appropriate evidence.

Here are two examples of the application of this conclusion, one positive, the other negative. It is asserted in the science of physics that in a closed system where there are exchanges of energy, energy is conserved. I am aware that there are competent

men who question the conclusiveness of the evidence supporting this rule. That does not concern me. What does concern me is that it be seen that this rule is not a principle of causality with universal application. For we have no knowledge of causality such that we can say that causation is possible only in terms of exchanges of energy in closed systems. So far as I can learn, we have reliable knowledge of a specific type of causal relations involving energy in both cause and effect, and in such situations, energy is conserved. The empirical evidence suggests the generalisation and supports it. If it does, it does. Period. Whether or not there are other types of causal relation, for example, one where energy is found in the effect but not in the cause, or vice versa, is another and an empirical question. That is the positive illustration.

The negative one is equally simple. Descartes, it is commonly agreed, held that an imperfect being could not of itself produce the idea of a perfect being, appealing to the principle that there must be as much reality in the cause as in the effect. Much as I admire Descartes, I can find no warrant for such a general rule about causation. What causes can produce what effects, can be settled only by performance. If I may be permitted an *obiter dictum* on Descartes' problem, I should say that in it only two things are clear, all else obscure. The first thing that is clear is that we are imperfect; the other is that we men have in our possession no *a priori* knowledge of causation prescribing what we can and cannot produce in the way of ideas—or of anything else for that matter. Reliable knowledge of causal relations can be and is established in the actual situations in which they operate. What is then found, is found; what not, not.[1]

To return to Hume, there remains for consideration that the principle of universal causality lacks both *a priori* and *a posteriori* proof: we are not in any position to say, 'Every event has a cause'. Yet we do know that some events have causes, and that others as causes produce certain other effects. This knowledge is limited, but it is knowledge. Hume was right about the principle, wrong about us. We are neither as ignorant nor as bamboozled as he tried to make out. Yet his being right about the principle has a significance for us we have largely failed to grasp. Granted both that we know some causal connexions and

[1] It might be added that there is adequate *empirical* evidence that imperfect beings such as we men are cannot by their own efforts make themselves perfect. It is our causal capacity with reference to the production of ideas of perfect beings which still seems unsettled.

that the universal principle lacks proof, we should then be ready to find in experience not merely two possibilities but at least three. The first is the ordering of events in terms of causation involving exchanges of energy. And we do find some of these. The second is the occurrence of events with a *de facto* orderly pattern yet lacking causal origin and connexion. The existence of such events is debatable. The third is the connexion of events in a settled, reliable fashion yet the connexion is not interaction. This third possibility readily permits verbal juggling, not all of it perverting. One way of describing this possibility is to say that while all cases of compulsive or energy determination are cases of cause and effect, not all cases of cause and effect need be cases of compulsive determination. Personally, I prefer to say that there may be a relation in which there is a settled and reliable causal connexion and yet no *interaction—i.e.*, no *exchange* of energy. In any case, whether this possibility, however named, is actual, remains to be seen. And that is an empirical question.

I now come to my second thoughts on Berkeley. As I have already said, the so-called refutations of Berkeley on careful examination are found to be no refutations at all. On the other hand, an equally careful re-examination of Berkeley disclosed that his own proofs against matter are likewise inconclusive. Berkeley does prove a number of important things—he proves, for example, the inseparability and equal epistemic standing *in perception* of primary and secondary qualities—but he does not *disprove* the existence of material substance. The inconclusiveness alike of the argument *against* materialism and *against* idealism, both terms being used in the sense appropriate to Berkeley, indicates that physical objects, or alternatively God, as the independently existing cause of our external perceptions, are interpretations of the data of experience. Berkeley admits as much when he *argues* that God, being active, is a more likely and comprehensible cause of the content of perception than inert matter could be. This is to say, in Berkeley's terms, that neither God nor material substance is the object of a simple perception: we perceive tulips or strawberries as the case may be. Physical objects or God, as the independently existing causes of our perceptions, have epistemic standing as interpretations for which arguments pro and con are offered. Berkeley's own arguments in favour of God appeal to causation in two distinct ways, first, to the general principle of causality, second, to specific considerations relevant to the appropriate cause in a particular kind of causal relation. These two arguments must be kept distinct.

Starting with the first, we find that Berkeley recognises, as completely as any realist, that when we open our eyes, we see not what we will, but what is present in our visual field. To account for this, he tells us, there must be a cause *other than what is perceived*. What we perceive has its being in being perceived, yet *that* we perceive *this specific* 'what' rather than another, is not in our power to determine, and therefore must be determined by something else. This is, at least in part, an appeal to the general principle of causality ,'every event has a cause'. Such an appeal is, as we should now know, invalid, because of Hume's demonstration that this principle has itself no independent standing. Of course, as Hume usually forgets, the contradictory principle, some events have no causes, has no independent standing either. But I shall not go further into this question, for it is in fact the general problem of the justification, if any, of the belief in an external world. One point only I wish clearly noted. The view that our perceptual experience is *of* an independently existing source is, in its generic form, a view on which Berkeley, Dualists, and modern Realists agree. They agree there is *some sort* of independent reality—physical things, God, or something else.

What particular kind of external reality we are to believe in, is the next question. Berkeley rejects a material world and proposes God. His reason, in brief, is that God is by nature active, matter inert; God as mind or spirit is like unto us in whom the perceptions as effects are to be produced, matter is different in kind. In this Berkeley directs our attention to the differences between matter and mind, physical things and ideas, and then he takes these differences to be evidence for the impossibility or improbability of any causal activity of mind with matter, matter with mind.

There are two elements in this argument against matter.[1]

[1] In appraising this and the next few paragraphs it should be remembered that this essay does not claim to be a complete examination of Berkeley's philosophy. In particular, Berkeley's polemic against abstract ideas is not considered because such consideration is not necessary for the subject in hand. Yet even in a footnote this much can be stated. Berkeley's polemic proves that we cannot have either in perception or in imagination an *image* of figure in general: images are particular. Descartes had noted that fact before Berkeley. But Berkeley does not prove either (*a*) that we have no knowledge of universals—this is a commonplace—or (*b*) that we have no knowledge of, to be specific, figurateness whose specific character is not to be identified with any one of the sensory qualia, such as colour, found in our sensory images of particular figures. *Seen* figures, in perception or in imagination, are *coloured* figures. But no one has ever demonstrated that our knowledge of figurateness must be in visual terms, or tactual

The one is the contention that matter has a specific, essential character, inertness. The other is the reliance on an alleged causal rule: matter and mind, because unlike in kind, cannot causally interact. Let us first consider the rule. It may be understandable that Berkeley regarded that rule about causes as valid. But for us to do so after Hume's work, is to disclose a singular ignorance and stupidity. As has previously been brought out, we have no general rule about what kind of thing can interact causally with what other kind, or what cause can produce what effect. Prior to actual performance no man knows what can cause what. Matter cause an idea? Mind control body? Why not? The only proper question is neither why not, nor why, but simply, does it? And to get any answer we must reverse Berkeley's order. We must first determine whether or not there are physical bodies and mental ideas, whether or not mind is active, matter inert. If such questions are answered in the affirmative, we can then look for empirical evidence relevant to the question, does inert matter cause ideas in active minds?—do minds determine motions in bodies? That order of questions, and precisely those questions in that order, are the clues to reliable conclusions in this total situation. To discover that was for me an important achievement of my second thoughts.

I now direct attention to Berkeley's assertion that matter is inert. Strictly this was not his own view but that of those who believed, as Berkeley did not, that matter has real existence. He took it from them and then used it as an argument against that belief. In the taking he was certainly justified. The inertness of matter was a cardinal doctrine of the physical science of Berkeley's day, in fact, if not in word. It was given definitive expression in the Newtonian law "every body continues in its state of rest or motion in a right line unless acted upon by an external force". The use of the term 'motion' should not deceive us, nor should a shift to such terms as energy or electricity. The crucial phrase is "unless acted upon by an external force". If that holds, then matter is in itself perfectly inert. At rest, it cannot begin to move; in motion, it cannot stop, cannot swerve, cannot even slow down—of itself. Something external is required. Berkeley was completely right in saying that anything thus described is inert. I, myself, am not one of those philosophers who readily challenge the truth of the pronouncements of science. Towards science I am as a dutiful child to an

terms, or any other specific sensory qualia. There is evidence that the contrary is the case: that we can *know*, not sense, figure in terms of sheet boundaries in pure space.

eminently respectable adult, accepting with deference all pronouncements on matters of fact, no matter how extraordinary they may appear. It is not questioning its truth to make the following notations on this scientific account of matter as inert. The empirical evidence offered by science to confirm its law is derived from carefully selected and restricted situations, and the theoretical considerations are similarly restricted in their reference. In consequence, the inertness of matter is well established as regards the behaviour of physical bodies in a specific type of situation—exchanges of energy in a system containing only physical bodies—but there is no warrant for claiming that this inertness is an exhaustive account of the capacity of matter in all situations. Precisely as in the case of causation, we have a generalisation which is true in its proper reference ; but the range of this reference may not be coincident with the entirety of the empirical world. Further, the inertness of matter in the scientific sense is strictly formal. That is one reason why it is so useful to science and so deceiving to ordinary people. This formal inertness assures science that no physical body will ever of itself upset an equation by showing up with a velocity value on one side underived from the other ; that energy systems will maintain themselves *as a whole* if there is no *interruption*. This inertness is perfectly compatible with ceaseless jumpings, reboundings, even atomic explosions. Finally, I call it formal because this inertness is not to be identified with, although it has a relation to, perceptual or sensory inertness.

Sensory inertness brings us to what Berkeley did say for himself, that ideas are inert. Disagreement with his final conclusions or awareness that for us there is a persistent ambiguity in his use of the term idea, should not blind us to the correctness of Berkeley's observations. Resistance to our efforts, adverse space occupancy, what Dr. Johnson intended us to understand when he dashed his foot against a stone—and the Bishop certainly needed no instruction from the Doctor—these are the particulars of the inertness Berkeley and we certainly find in our commerce with things. This inertness is crucial in one of Berkeley's arguments. Inertness, accepted as an essential character, gives positive content to the term ' material substance ', which otherwise is as empty as Locke admitted when he said it was a something he knew not what. But Berkeley believed that extended, inert matter could not be the causal source of our perceptions. It has been one of the primary purposes of this essay to indicate that there is no warrant for that view of matter. Let material substance be extended and inert, that settles nothing as to its

causal capacity with reference to minds. Only the trial of performance can be decisive. With that seen clearly, my second thoughts, guided by Berkeley but attending strictly to the empirical evidence, brought me to the conclusion that material substance exists, with inertness as one of its chief attributes. This, for me, meant dualism. I have not felt it necessary in this essay even to state that I regard myself as certainly real, a thinking substance. First thoughts, guided on occasion by St. Augustine, Descartes and Berkeley, suffice for that. Only for the existence of material substance were second thoughts required. But assiduous reflection on the evidence did achieve that result. Mind and matter are different in kind, both are substantial, both are real.

But that is not all. The inertness of matter is bound up in fact with the type of causation involving energy exchanges, causation by the application of *external* force. Instances of such causation can be discovered. There are also situations in which something else is found. Attending particularly to inertness and causation in terms of forces, I discover a constant and crucial difference among bodies. Bodies are inert in relation to other bodies and to my body, and my body to them: any change of motion in one requires energy from an external source. But *my* body is not inert to *my* mind, nor *my* mind to *my* body. All the genuine empirical evidence I can get indicates that my body is responsive to my will, and my mind responsive to my body. The relation is causal, but not interactive. No motion, no energy is transferred. To a volition in the mind the body responds by what is appropriate to its mode of existence, namely movement. The energy involved is the body's own, none comes from the mind, for the mind strictly has no energy. To a motion or energy state in the body, the mind responds by what is appropriate to its mode of existence, namely feelings, sensations, perceptions. Then the activity is the activity of the mind, none comes from the body, for the body strictly cannot think or feel. Yet this relation is causal. Volitions determine and control bodily motion. The body, *inter alia*, determines and controls what the mind feels and perceives. Mind and body, each without loss of essential differences, exercise causal control each over the other. The control is limited; it is specific in that the effects are always those appropriate to the kind of substance in which they occur, motions in matter, ideas in minds; but the causation is genuine, actual.

Against this, the conclusion of this set of second thoughts, what evidence can be brought? That matter and mind because different cannot have causal connexion? There is no such rule

or principle established on *a priori*, rational, or *a posteriori* grounds. The genuine empirical evidence is all the other way. The law of the conservation of energy is not relevant, and in any case it is not contravened. It is no objection that this imputes to matter a new quality, quite different from inertness. What if it does ? It leaves untouched the inertness of bodies *vis-à-vis* other bodies, the only situation in which inertness is verified or important. This view asserts a character I have called " responsiveness " of *both* bodies and minds in the specific relation of living union. It does so on the basis of empirical evidence.

On that note my second thoughts rest. I care little for labels, but for those who like them I shall label this position. It is resolute empirical rationalism. It is resolute because it is not frightened by names. It is empirical because it holds to the evidence. It is rational because it is the result of reflections judged by the criterion of truth, and because it makes sense.

V.—DISCUSSION.

A NOTE ON LOGIC AND EXISTENCE.

In reference to Mr. E. J. Nelson's recent article on "Contradiction and the Presupposition of Existence" (MIND, October 1946), I want to comment upon a fundamental assumption involved in the author's arguments which I find highly questionable. The following is the paradox for the solution of which Mr. Nelson elaborates a distinction between "the necessary conditions of the existence of a proposition" and "the necessary conditions of its truth (exclusive of its existence)". fa implies $(Ex)\,[fx \vee -fx]$, but $-fa$, which is the apparent contradictory of fa, implies the same existential proposition. This common implicate, however, could conceivably be false, *viz.*, in case no individuals at all existed. Hence, by transposition, both fa and $-fa$ could conceivably be false; and since contradictory propositions cannot conceivably be both false, fa and $-fa$ are not contradictories. Mr. Nelson's solution of the paradox, if I have understood his arguments correctly, is that implication, as a logical relation, must be construed as a relation between what propositions *assert* (and propositions assert the necessary conditions of their truth); but fa does not, according to Mr. Nelson, *assert* that any individual (specifically, the individual a) exists, it only *presupposes* the existence of a as one of its constituents. Mr. Nelson's conclusion seems to be that fa does not imply the existence of at least one individual in the same sense of "implies" in which, for example, $(Ex)\,fx$ implies $-(x)\,-fx$. The existence of at least one individual is rather "presupposed" by the *existence* of the proposition fa; it is not a truth-condition of fa in the sense in which, say, g_a would be a truth-condition of fa, if $fa \supset g_a$ were true.

Now, as far as I can see, Mr. Nelson has not justified his assumption that, in asserting $(Ex)\,[fx \vee -fx]$, the existence of at least one individual is asserted and that, therefore, the above existential statement expresses a contingent truth which "is not certifiable on the grounds of logic alone". Let us first consider a most puzzling consequence of Mr. Nelson's assumption. If "$(x)fx$ implies $(Ex)fx$" is, as in Russell's logic, accepted as a transformation rule, then the debated existential statement follows from its corresponding universal which is *tautologous:* $(x)\,[fx \vee -fx]$. But how can a contingent truth follow from a tautology? There seem to be three ways in which the consequence that contingent truths may be entailed by tautologies could be avoided:

(1) To abandon the stated transformation rule.
(2) To interpret logical truths, in Mill's fashion, as simply the most extensive empirical generalisations.

(3) To deny Mr. Nelson's assumption that $(Ex)[fx \vee -fx]$ asserts the existence of individuals, and to maintain that this existential statement, like its corresponding universal, is purely tautologous.

It seems to me that (3) is the most plausible and least burdensome alternative to take. We only have to translate the statement "$(Ex)[fx \vee -fx]$" (where "f" is supposed to be a predicate constant) into the language of the class calculus, to see its purely tautologous character. For the corresponding statement in the class calculus is: the universal class (*i.e.*, the class which includes every class) * has members. But to say the universal class has members is simply another way of saying that it is different from the null class, which is surely tautologous.

The belief in the contingent character of an existential statement of the sort under discussion might arise as follows. Any existential statement can be formally exhibited as the implicate of a singular statement which verifies it. Thus, $fa \vee -fa$ implies $(Ex)[fx \vee -fx]$. The transformation rule here used is: fz implies $(Ex)fx$ (where "z" is the name of some unspecified individual). However, any existential statement may also be exhibited as the implicate of a universal statement, with the help of the transformation rule: $(x)fx$ implies $(Ex)fx$. Now, in formal logic, the predicate variable "f" may take on as values either empirical predicates (such as "red") or logical predicates (such as "red or not-red"). Obviously, those existential statements whose predicate is of the empirical variety, cannot be known to be true in any other way than by verifying at least one of the singular statements which each entail it; unless at least one zebra had been observed, the hypothesis "there are zebras" could not be asserted as true. This is the kind of consideration which naturally leads to the translation of existential statements into logical sums (finite or infinite) of singular statements. However, it is equally obvious that an existential statement whose predicate is of the logical variety (examples: $(Ex)fx \vee -fx$, $(Ex) - (fx \,.\, -fx)$), is not asserted as true on the ground of the observation of individuals which exhibit it. It is asserted as a mere corollary of the corresponding universal statement which is a logical truth and in which descriptive empirical predicates have a purely vacuous occurrence (*i.e.*, the truth-value of the statement would not change if any other empirical predicate were substituted instead).

That existential quantification, in formal logic, cannot be regarded as indicative of the assumption that at least one individual exists, is furthermore evident from the following consideration. Suppose we construct the concept of a kind which, as far as we know, has no instances. We may then draw analytic consequences from our arbitrary definition and formulate them as conditional statements.

* It should be understood that the reference is to classes of individuals, *i.e.*, classes designated by class symbols of type 1.

To illustrate, let "fx" stand for "x is a goblin", and "gx" for "x is imponderable". Then we may assert $(x)\ [fx \supset gx]$, as an analytic truth, assuming that imponderability is a defining characteristic of goblins. But now it may occur to us to subdivide the empty class of goblins into two likewise empty classes, *viz.*, the variety of pale goblins, say, and the variety of pink goblins. Let "hx" stand for "x is pale", so that we can assert: $-(x)\ [fx \supset hx]$. But this universal statement is equivalent to the existential statement: $(\mathrm{E}x)\ [fx\ .\ -hx]$. By the principle of simplification, it follows that $(\mathrm{E}x)fx$; in other words, from an arbitrary definitional construction of an empty class and likewise empty subclasses thereof, we seem to have proved that the defined class is not empty after all: there are goblins!

A clever theologian might exploit arguments of this sort in order to show that there is nothing logically objectionable in the assumption of the "ontological" argument that "essentia" may involve "existentia". However, the proper conclusion to be drawn is that existential quantification, in formal logic, is a purely formal operation which has no "ontological" import whatever. As the above sample argument shows, "$(\mathrm{E}x)fx$" cannot be interpreted to assert that the property f has empirical instances. f might be a definitionally constructed property determining a subclass of a class determined by a more abstract or less complex property. Thus, "$(\mathrm{E}x)fx$" might assert that there are isosceles triangles—besides other varieties of triangles—, without carrying the implication that any empirical instances of the concept "isosceles triangle" exist. Hence, if the individual constants "a", "b", etc., are regarded as proper names of empirical particulars, "$(\mathrm{E}x)fx$" can hardly be said to be *synonymous* with the logical sum "$fa \vee fb \ldots \vee fn$".

Mr. Nelson argues that "a exists" is a common implicate of fa and $-fa$. Whether the former proposition be regarded as contingent or necessary, he continues, at any rate it implies "$(\mathrm{E}x)\ fx \vee -fx$", which is itself contingent. Langford's interpretation of this situation is alleged to have been that fa and $-fa$ are not contradictories and that, indeed, singular propositions have no formal contradictories at all. Mr. Nelson, instead, concludes that "a exists" cannot be said to be implied by what "fa" *asserts*. There are three final comments I wish to make.

(1) Mr. Nelson seems to assume throughout his discussion that the sentence "a exists" expresses a proposition. But which are the constituents of this curious proposition? Presumably the individual a and the property of existence. However, we simply misuse language if we ascribe existence as a property to an individual; we can properly ascribe it only to a *kind* of individuals. It makes sense to say "zebras exist", but if someone pointed to an individual and said "this zebra exists", he could at best mean that what he sees is a real zebra, and that he does not suffer from an hallucination. But even so, he would not be ascribing a property, named "reality"

(which may or may not be taken as identical with existence), to the individual he sees; he would rather be making the meta-linguistic assertion that the assertion "this is a zebra" (where the *designatum* of "zebra" is, of course, the class of physical zebras, not the class of mental images of zebras) is true. Mr. Nelson does not seem to have considered Russell's demonstration that the logical analysis of sentences having "existence" as their grammatical predicate leaves you with propositions which do not contain existence as a constituent. Thus, in Russell's analysis, "zebras exist" would mean "the sentential function 'x is a zebra' is true for some values of x" or "the class of zebras is non-empty".

(2) Why not argue that $(Ex)\,[fx \vee -fx]$ must be a tautology *just because* it is implied by contradictory premisses? Mr. Nelson affirms that "no two propositions are contradictories if they have a common implicate, regardless of the status of that implicate". But is it not an established theorem of formal logic that a tautology is implied by *any* proposition, just as a self-contradictory proposition distinguishes itself from self-consistent propositions by the fact that it implies any proposition? To say that p entails q is to say that $p\,.-q$ involves a self-contradiction. Hence, if both p entails q and $-p$ entails q, $p\,.-q$, as well as $-p\,.-q$, must involve contradictions. But if q is a tautology, then $-q$ is a contradiction hence the condition of entailment is satisfied in both cases.

(3) Mr. Nelson might reply that, if apparently contradictory premisses have a common implicate which is contingent in the sense of being conceivably false, then either they are not really contradictory (Langford's conclusion which Mr. Nelson is "reluctant" to accept), or they do not imply that contingent proposition in the ordinary sense of "implication", *i.e.*, the sense in which implication is a logical relation between *assertions*. This seems, indeed, to be Mr. Nelson's own conclusion. The alternative, however, which he has not considered is, that the common consequence $(Ex)\,[fx \vee -fx]$ *could not* be false, being a logical truth. I shall now present my final argument for this alternative. According to Mr. Nelson, the above existential statement implies that individuals exist, which could be false. But what is the logical status of the statement "individuals exist", what is its logical form? It seems to have the same form as the statement, say, "lions exist", and since the latter would in Russell's logic, which does not recognise "existence" as a *predicate*, be rendered as (Ex) lion_x, "individuals exist" might be formalised in the same fashion: (Ex) individual_x. It thus *looks* like a contingent truth, since, just as we might find no individual verifiers for (Ex) lion_x, we might, as it seems, find no x's which have the property of being individuals. Thus, "a is an individual" looks like a contingent truth, of the same character as "a is a lion". But upon closer inspection it turns out to be an example of Carnap's "pseudo-object-sentences"; the "parallel syntactic sentence", from which it is inferrable without any factual inquiry into the extra-linguistic

referents of language, is: "*a*" is the name of an individual. If I assign the name "*a*" to an individual, it still remains logically possible that *a* should turn out not to be a lion. But it is logically impossible that it should fail to be an individual. But if "*a* is an individual" is not contingent, then its implicate "individuals exist" cannot be contingent either. It is, indeed, natural to ask: Is it not logically possible that individual constants should remain without referents? But this question arises from a confusion between individual constants (which are proper names) and predicates (which are general names). Predicates are names of properties (or relations) which may or may not have individual instances, but individual constants are, by definition, names *of* individuals.

The same kind of objection may be urged against Mr. Nelson's assertion that (EP) $Pa \vee -Pa$ could be false, on account of asserting the existence of properties. "*f* is a property", like "*a* is an individual", is a pseudo-object sentence; its "syntactic parallel" is: "*f*" is a predicate. A world of individuals devoid of properties is a logically impossible world. We may be mistaken in believing that a given individual has a given property; it may turn out that it has a different property and, indeed, that no individual at all possesses the first property. But it is logically impossible that an individual should fail to have any properties at all. For "*a* has no properties" implies that *a* has the property of having no properties, which is self-contradictory. In other words, if there is no way of describing a given individual, it can at least be described as undescribable.

ARTHUR PAP.

University of Chicago.

VI.—CRITICAL NOTICE.

Philosophic Foundations of Quantum Mechanics. By HANS REICHENBACH. Berkeley and Los Angeles: University of California Press, 1944. Pp. x + 182.

THE review copy of this book, which was sent to me in April, 1945, did not reach me; I regret that not until October, 1946, did I have access to another copy. The delay in the appearance of a review is partly offset by the value of the book, which is by no means ephemeral; on the contrary the work is likely to be of importance for some considerable time.

Part I is short and explains the author's ideas. Part II is a mathematical exposition of quantum mechanics, which, both in spite of and because of its relatively brief compass, should prove useful to many readers: it is too short to be a treatise, but it is longer than a mere outline—it is in fact a comprehensive sketch. Part III deals with the application of the mathematics to experimental results and shows how the interpretations of quantum phenomena emerge; it also contains an exposition of three-valued logic, and shows how quantum results can be expressed in terms of this—surely a highly important contribution. Although mathematics fills a large part of the book, the persevering general reader could probably make more of this fascinating and queer subject from this work than he could from the popular presentations that abound. The author has evidently taken a good deal of trouble with his exposition, to bring out the underlying difficulties and subtleties rather than to shun them. The aim of the work is critical clarification; no attempt is made to intrude into the physicist's domain or to make a positive contribution to physics.

The commotion caused by modern physics centres, of course, around Heisenberg's Uncertainty Principle. This means that, where more quantities than one are needed to describe the state of something it is impossible to get simultaneously exact values of these quantities—in the usual illustrations it is impossible to get exact values for position and momentum (or velocity). In the course of exposing many misconceptions of the principle, Professor Reichenbach stresses that it is not enough to attribute this state of affairs to the disturbance caused by instruments of measurement; for in classical physics instruments disturb what is measured, yet without making the values obtained inexact—thus when a thermometer is placed in a calorimeter it disturbs the measurement of calories yet an exact correction can be made. The essential point about the Uncertainty Principle is that exact allowance cannot be made for disturbance.

Another way of putting the same general point is that for classical mechanics there is such a thing as a "normal" system, which is questioned by quantum mechanics. Taking what is observable as a

phenomenon with sufficient latitude to allow electrons and trees this status, Professor Reichenbach introduces the word "interphenomenon" for what is unobservable, such as trees when unperceived; thus the interphenomenon is an interpolation. Many systems of interphenomena could be devised to make a coherent picture of phenomena. There is, however, no way of choosing between these systems or of saying which one is true. The systems of concepts used therefore constitute *equivalent descriptions*, and the one adopted is the one with the greatest *descriptive simplicity*. Now a "normal" system is characterised ultimately by one thing: it must be such that the laws of nature remain the same whether or not objects are observed. And according to classical mechanics it is possible to find a normal system of interphenomena or a single description, while this is denied by quantum mechanics. The best way to be clear about this is to focus on certain fundamental experiments, *e.g.*, experiments having to do with interference.

Let a ray of light pass through a slit on to a screen; instead of a sharp image, we find an "interference pattern". If light of low intensity is used, we see flashes, and, if we kept a sensitive film on the screen for a short time, the record of the sum of the flashes would appear as the interference pattern. Hence we propose the description: light consists of corpuscles. However, we could also say that light consisted of waves, provided we agreed to say that the whole of a wave apart from the point where the flash occurred disappeared when the wave struck the screen. This would be just as correct a description of the interphenomena; the phenomena are the same on either interpretation—the flash, for instance, verifies neither. There is one and only one objection to it: it introduces a *causal anomaly* (the disappearance of the wave). Now let there be two slits, A and B, and flashes at C. On one film take a record of the flashes passing through A when B is closed and passing through B when A is closed. The result should be two interference patterns obtainable by keeping A and B open simultaneously? Obviously. But the experimental fact is otherwise. Therefore the behaviour of particles or corpuscles passing through A depends on whether B is open: there is action at a distance—a causal anomaly. (In this experiment the wave interpretation gives rise to no difficulty.)

Hence we see that in some instances causal anomalies are due to the corpuscle and in others to the wave interpretation. In other words there is no unique description (unless of course light consists of something that is neither a corpuscle nor a wave); there is no normal system of interphenomena. But, though there is no one description for all circumstances, there is always a description for any circumstance. Professor Reichenbach illustrates in this way. If you wish to describe positions on the globe by means of circles of longitude, there is no unique longitudinal framework in which every point on the Earth will have a definite longitude—the North and South Poles have none in the ordinary geographical framework.

But you can assign a longitude to the Poles by shifting the longitudinal framework, so that the great circles of longitude all intersect at two other points on the Earth. Thus there are longitudinal descriptions of any point but not one for all points. In microphysics sometimes the corpuscle description will serve, sometimes the wave. Either can be used if you do not mind causal anomalies. This has been expressed as the Principle of Complementarity by Bohr: *i.e.*, either is usable, neither is verifiable. We need not spend time on such subtleties as the pilot wave riding on a corpuscle, so as to have the best of both concepts; this involves "waves" without energy but is for all that usable. It is a question, then, of adopting the description that is easiest to use, or that is preferred for any reason; but there is no acid test. In short, if any one interpretation is maintained all the time, causal anomalies must occur: "It is impossible to give a definition of interphenomena in such a way that postulates of causality are satisfied. *The class of descriptions of interphenomena contains no normal system.*" This Professor Reichenbach calls the Principle of Anomaly.

So much for what, I believe, is an extremely telling exposition of the situation at the Quantum Conference. We turn to well-known interpretations of this.

One may rest content with a duality of interpretations—now corpuscle, now wave. And one may be satisfied with causal anomalies. If not, since there is no exhaustive interpretation, we may try those that are restrictive. The positivist form of restrictive interpretation, whether rightly or wrongly attributable to Heisenberg, dispenses with descriptions of interphenomena as meaningless: thus we may not say that if the particle went through the slit A it must have been influenced by the slit B. Professor Reichenbach thinks this over stringent: it has the merit of avoiding the paradoxes of causal anomaly, but while working with the wave concept (or corpuscle) it forbids us to ask questions that are quite sensible within the wave (or corpuscle) framework. He therefore prefers another form of restrictive interpretation. Instead of saying that the answers to certain questions are meaningless or that there are no answers, he shows it is possible to hold that there is an answer and that the answer is *indeterminate*. This does not mean "exact but unknown (or unknowable)"; it has the sense that one value is as good as another—perhaps, if we do not stretch the parallel too far, like the fraction 0/0. It is in this sense that he speaks of Heisenberg's Principle as the Principle of Indeterminacy. Some writers are inclined to prefer to call it the Uncertainty Principle because "indeterminacy" in some contexts implies psychological considerations about spontaneous freedom; but "uncertainty" also has its psychologically irrelevant meaning, and "indeterminacy" is perfectly satisfactory as Professor Reichenbach uses it.

Now this approach leads the author to a three-valued logic, where the truth-values are true, false, and indeterminate—so that, for

instance, the denial of p does not mean that p is false. He develops the calculus of this to show that within this language quantum mechanics can be fully expressed, that causal anomalies do not arise, and yet that it is meaningful to ask the kind of question most of us like to ask—thus certain statements are rescued from the Heisenberg *Index*. This seems to me a valuable undertaking. (Incidentally it leads to an interesting treatment of an argument between Einstein, Podolsky, and Rosen on the one side and Bohr on the other, pp. 170-176.)

Let us summarise the position. (i) If you do not mind causal anomalies you may stick to corpuscle interpretations always (or wave always). Most people do mind them. You can avoid them (ii) by using the corpuscle or wave interpretation according to circumstances, or (iii) by putting certain statements on the positivist *Index*, or (iv) by introducing a three-valued logic. It cannot be too strongly stated that the only reason for any of the last three alternatives is the dislike of causal anomaly. But there is a price to pay: with (ii) you relinquish a normal system ; with (iii) you are deprived of the right to ask certain questions, and with (iv)—Professor Reichenbach is quite open about it—you are left in the air with the concept *indeterminate* which is bound to be unsatisfying to the majority of people. One of the merits of the work is the force with which the author shows how the Uncertainty Principle binds one hand or the other—freeing one shackles the other.

There is almost nothing in the book that I can find to criticise—not because I like the conclusion, for I dislike it, and I think the author does so too. There are a few remarks, however, that may not be out of place.

Analysing the situation into four interpretations—or any other number—what is the difference between them ? They all fit the known facts. Indeed they must fit the facts because they are essentially concerned with interphenomena. Surely, then, they are all equivalent statements ? Does it not therefore come to this: that the kind of interpretation adopted by a certain thinker defines his like and dislike of causal anomaly and so on ? No matter which one is adopted some quantum trouble persists ; it is not a single one for all interpretations, but there is some trouble for any interpretation. Thus the kind of interpretation you choose depends on the kind of troublesome question that does not disturb you over much.

Regarding the possibility of reducing quantum mechanics to "a statistical part of science imbedded in a universal science of causal character", *i.e.*, finding a new interpretation of interphenomena that shall give a normal system without causal anomaly, Professor Reichenbach writes that although "we cannot adduce logical reasons excluding such a further development of physics, and, although some eminent physicists believe in such a possibility, we cannot find much empirical evidence for such an assumption" ; for we argue inductively that since the Uncertainty Principle holds in spheres so far

considered it will hold in all others (p. 14). This is a very mild argument. It leaves room to hope for a new interpretation free from paradox. But perhaps natural selection will see to it that the coming generation will feel no sense of paradox or frustration in the presence of causal anomalies or indeterminacy.

A small point: would it not have been a good thing in the general exposition of his ideas for the author to have discussed the statement that with quantum predictions all values, say of momentum, within a certain range are equiprobable, and to have discussed the sense of "equiprobable"?

One of the minor but by no means unimportant merits of the book is to state two alternative definitions concerning measurement, hitherto overlooked, but unwittingly used by opposing parties in quantum disputes (p. 118 and p. 140).

The author puts forward the interesting view that the common-sense laws of nature could be satisfactorily replaced by others in the following curious way: on observing a shadow, usually said to be due to a tree, we could say that it was the effect of two trees throwing coincident shadows, although on inspection only one tree was visible. I feel this would require a long discussion and take us too far from the essential theme of the book.

In general, philosophers concern themselves with paradoxes arising from facts that come under their observation. It is important that they should be alive to the paradoxes arising from quantum facts. Such facts cannot be shelved as merely technical or as belonging to a special department; they are facts along with all the other more familiar facts about nature; they are no less real because revealed by complicated laboratory apparatus than are those revealed by the human eye.

J. O. WISDOM.

VII.—NEW BOOKS.

Kausalität und Zufall in der Philosophie des Aristoteles. By Hélène Weiss. Verlag Haus zum Falken, Basel, 1942. Pp. 193.

The declared aim is an interpretation of Aristotle's treatment of chance in *Physics* B 4-6. The Vorwort acknowledges that a genaue Besprechung of the text of these chapters is still wanting. The Nachwort claims that our preparation for an interpretation of these chapters has been completed. The book is only part of what the title represents it as being.

Does not the plan foredoom the work to failure? Miss Weiss's order is not Aristotle's. Not that the editor's decision, little as Miss Weiss thinks of challenging it, is final. But B 7-9, especially 8, is plainly an application of B 4-6, especially 5. The argument of 8, that the natural is not casual but *ἕνεκά του*, is addressed to readers already familiar with the doctrine of 5, that chance is the furtherance or hindrance by one agent of the *οὗ ἕνεκα* of another. Interpreting 4-6 in the light of 7-9, in the light of what does Miss Weiss interpret 7-9?

Conceding that rain by chance makes or mars the crop, Aristotle denies that teeth come up fitted by chance, some for tearing, others for grinding. Miss Weiss sees the peculiarity of the rain-corn illustrations, namely that "an in itself foreign occurrence has decisive significance . . . for the attainment of the end of another existent" (p. 86). But she misses the relevance of this peculiarity. For she proceeds: "Analogous in principle to these connexions are found those relations that meet us within a single living being." Aristotle's distinction of the *ἕνεκά του* from the casual is precisely the rejection of this supposed analogy. "Alle diese Sätze sind aus der Haltung desjenigen gesprochen, der das Umwillen an der Natur leugnet" (p. 88). Not those about rain and corn. Only that about teeth.

Aristotle's doctrine, that chance is the furtherance or hindrance by one agent of the *οὗ ἕνεκα* of another, is an application of his doctrine, that accident is the interference of one agent with another. Miss Weiss has not yet published her interpretation of Aristotle's treatment of chance. But she has already published her interpretation of his treatment of accident.

She rightly rejects the statistical interpretation, as of *ἐπὶ τὸ πολύ*, so of *οὔτ' ἐξ ἀνάγκης οὔτ' ἐπὶ τὸ πολύ*. But I cannot concede her claim that, from this privative conception, she has, with the help of *Metaphysics* E 2, pushed forward to the positive nature of accident (p. 181). Nor do I find the claim consistent with the obligatory admissions by which it is jostled (pp. 173, 180). Matter is neither more nor less the cause of the accidental than of the prevalent. To find in matter the whole cause of the accidental is not to find its positive nature, but to find that it has none.

The quarry lurks rather in that one, of the formulæ of *Metaphysics Δ* 30, in which 'not' is balanced by 'but': *γέγονε μὲν δὴ ἢ ἔστι τὸ συμβεβηκός, ἀλλ' οὐχ ᾗ αὐτὸ ἀλλ' ᾗ ἕτερον*. This sentence plainly belongs to the illustration *εἰς Αἴγιναν ἐλθεῖν*. Without reference to the context, Miss Weiss urges the translation "aber nicht sofern es es selbst, sondern sofern es ein Anderes ist" (p. 163). Reading *τῷ* rather than *τὸ* at 1025 a 26, I prefer the Second Edition of the Oxford translation: "not in virtue of the subject's nature, however, but of something else."

Only by main force can Miss Weiss impose her interpretation on Aristotle's illustrations (p. 189). Neither the plea that gold is not there as gold nor

the plea that Aegina is not there as Aegina would, even if plausible, suffice. Aristotle's illustrations of accident are not treasure and Aegina, but finding treasure and going to Aegina.

Rejecting the statistical interpretation of *ἐπὶ τὸ πολύ*, Miss Weiss rightly turns, for her true interpretation, to *ἂν μή τι ἐμποδίσῃ* (p. 93). "Nur durch die Möglichkeit hindernder Störungen, die nicht seiner eigenen Arche entspringen, hat sich das Naturding vom 'immer' entfernt gezeigt" (p. 166). Unfortunately Miss Weiss loses sight of das Naturding in an effort to see die Natur im Ganzen.

The prevalent conception of nature is negative. That is natural which is due to other than (human) agency. Aristotle's conception is positive: *ἕκαστον ἐν ἑαυτῷ ἀρχὴν ἔχει κινήσεως καὶ στάσεως*. Aristotle's *φύσει ὄντα* are simply agents. To make anything of this, you must distinguish one *φύσει ὄν* from another. Lose each *φύσει ὄν* in the totality of *φύσει ὄντα*, and you, missing the differentia of the Aristotelian conception, fall back on the prevalent conception.

Aristotle's unambiguous *ἕκαστον* Miss Weiss represents by "sämtlich," "alle" (p. 46). Her "in sich selbst," accordingly, although it is her representation of *ἐν ἑαυτῷ*, we must represent by 'in themselves.' Enough, therefore, if of the *κίνησις* of one *φύσει ὄν* the *ἀρχή* be in another. May the *ἀρχή*, then, be anywhere? Anywhere but in a human agent.

This is no merely syntactic lapse. Among Miss Weiss's illustrations one is decisive: "A stone disintegrates or is hollowed out by the river; it changes its shape untouched by human hand." Not untouched by the river. The river may have some claim to rank as an agent. The stone so far has none. To be hollowed out is not to act. But Miss Weiss so dissociates nature from agency as to say: "das, was 'von Natur' ist, die Arche dessen, was mit ihm und aus ihm wird, in sich selbst hat" (p. 51). No wonder that, alongside Baum and Tier, she includes Berg among *φύσει ὄντα* (p. 53).

Devoting nearly half her space to *Physics* A-B, Miss Weiss silently takes continuity for granted. She even finds "unerlässlich" (p. 44) the question "Wo liegt der Anlass, dass die Arche-Forschung eine solche Wendung nimmt, dass sie auf die Aitia stösst?" (p. 47).

Unlike Arche "kann die Aitia den ausdrücklichen Zusatz einer ersten erhalten" (p. 14). Among many passages, where *ἀρχαί* as well as *αἴτια* are thus qualified, is the opening sentence of the *Physics*.

Aristotle's refutation of *ἓν τὸ ὄν* is found circular because proceeding on the hypothesis *πολλαχῶς λέγεται τὸ ὄν* (pp. 21-22, 27-28). What Miss Weiss takes for a hypothesis is rather an admission, not alleviating but aggravating Aristotle's burden.

"Ausdrücklich macht Aristoteles gegen Sokrates geltend, dass es nicht genüge, zu sagen, die Arete sei die Haltung *κατὰ τὸν ὀρθὸν λόγον*, vielmehr müsse man sagen: *μετὰ τοῦ ὀρθοῦ λόγου*" (p. 140). Socrates went farther: *λόγους τὰς ἀρετὰς ᾤετο εἶναι* (E.N. 1144 b 29).

Metaphysics E "hat zum Ergebnis: *τὸ μὲν ὡς συμβεβηκὸς ὂν ἀφετέον*" (p. 156. *Cf.* p. 162). No acknowledgement of the omission of *καὶ τὸ ὡς ἀληθὲς* between *συμβεβηκὸς* and *ὄν*. The omission is material. For "das *ὂν ὡς ἀληθές* gleichsam den Gipfel der Seinsuntersuchung bildet" (p. 156, n. 9).

Everywhere insisting that the *φύσει ὄν* exhausts the *ὄν*, admitting only that "am *κινούμενον* zeigt sich eine *οὐσία ἀκίνητος*" (p. 67), Miss Weiss nowhere tells us what to do with *Metaphysics* E1, with *Λ*, or even with *Physics Θ*.

On the credit side? Lucid exposition and sustained relevance. At least. In the measure of my dissent, I may be alone. Not in the measure of my gratitude for a book which everywhere invites assent or dissent and nowhere leaves the reader befogged.

REGINALD JACKSON.

A Sketch of Mediaeval Philosophy. By D. J. B. HAWKINS. London: Sheed & Ward, 1946. Pp. vii + 148. Price 6s.

AT long last a British scholar has written an introduction to scholastic philosophy that can be recommended to readers both for the accuracy of its history and the soundness of its philosophy. Father Hawkins' book has three outstanding merits which deserve special remark even in a very brief notice.

Firstly, it gives a very fair and conciliatory account of the value of this study in the modern world. Father Hawkins does not believe that much is to be gained from a study of the scholastics, until one has found an affirmative answer to the epistemological questions raised by modern philosophers like Descartes and Kant. The present reviewer has not read his *Criticism of Experience*, but he presumes that in that work Father Hawkins has raised and answered those questions to his own satisfaction. There are many British philosophers who will agree heartily with this point of view, though they perhaps feel that the criteriology of the Neo-scholastics, which has certain affinities with Kemp Smith's interpretation of Kant's Second Refutation of Idealism, is less satisfactory as an answer to Descartes than the Philosophy of Common-sense, as formulated by Reid and Moore. The uncriticised assumption of Descartes is that we never know anything until we know that we know it—a dogma that results in Solipsism of the Present Moment. On such points there is genuine affinity between Moore and St. Thomas, who believed that when we apprehended ourselves, we apprehended the general adequacy of our faculties for their objects. Father Hawkins has a very clear understanding of the relations between philosophical inquiry and other branches of knowledge. He wittily expresses the difference between the point of view of St. Peter Damiani, who treated philosophy as the handmaid of theology, and that of the modern world, in the following way. 'Perhaps modern conditions suggest an even more accurate metaphor, for philosophy might be called the charwoman of theology. She has a house of her own to keep in order, but she is able and willing to spend a few hours a day in assisting the loftier Lady Theology' (pp. 14-15). A most just comparison, if we add that she has been practically blitzed out of her home by the Logical Positivists, and repeatedly goes on strike at being asked to dust the valuable, but fragile antiques of Fourth Century Symbolics.

Secondly, this introduction shows an excellent proportion. Although no aspect of scholasticism from Augustine and Boethius to Suarez and Molina is altogether overlooked, the chief emphasis is placed on the Thirteenth Century with a very valuable preliminary account of Anselm and Abélard, and a severe, but just, estimate of the Nominalist movement in the Fourteenth Century. Father Hawkins devotes separate chapters to Bonaventura, Aquinas and Scotus, and it would be hard to imagine a better short sketch of Thomism than is to be found here. His account of Roger Bacon is sympathetic, but too brief to do justice to his extraordinary

learning and keen scientific interest. The time has come when we must recognise that Bacon has a place in Thirteenth Century philosophy only second to that of St. Thomas: that had it not been for the overwhelming influence exercised by Aristotelianism after the completion of the *Opus Recentius*, scholasticism would have been diverted by the Oxford Franciscan to mathematical and experimental philosophy, an outlet which it eventually found in the days of Kircher, Mersenne and Cavalieri.

Thirdly, tribute must be paid to the remarkable Select List of Books, with which this book concludes. No one but a master of the subject could have compiled it; for, although it gives the most up-to-date information about sources and commentaries, it does so without burdening the memory with the name of a single book about which it has not a valuable comment to make. Though the reference to Steele's *Opera hactenus inedita Rogeri Baconi* is accurate, mention should also have been made of Bridge's standard edition of the *Opus Majus*, the *Opus Minus*, and certain parts of the *Opus Tertium*, of Charles' *Bacon* and of A. G. Little's *Roger Bacon Essays*. These, however, are very trifling matters, compared with the debt that every British scholar owes to Father Hawkins, and we hope that the public will quickly come to acknowledge him for what he undoubtedly is—the ablest exponent of scholastic philosophy who has yet written in the English language.

ARTHUR T. SHILLINGLAW.

Received also :—

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B. Croce, *Politics and Morals*, trans. from the Italian by S. J. Castiglione, London, G. Allen & Unwin Ltd., 1946, pp. 138, 8s. 6d.

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E. Roniger, *Gestaltendes Denken:* Vols. I and II, *Bindung und Entfesselung;* Vol. III, *Kosmos und Chaos*, Zürich, Verlag für Gestaltendes Denken, 1946, pp. 223; 247; 235, 35 frs.

M. Gex, *Initiation à la Philosophie*, Lausanne, F. Rouge et Cie. S.A., pp. 326, 6 frs.

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A. Masnovo, *Da Guglielmo d'Auvergne a S. Tommaso d'Aquino :* Vol. I, *G. d'Auvergne e l'Ascesa verso Dio ;* Vol. II, *L'Origine delle Cose da Dio in G. d'Auvergne ;* Vol. III, *L'Uomo*, Milan, "Vita e Pensiero", 1946, pp. vii + 192 ; 145 ; vii + 212.

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F. Olgiati, *Il Concetto di Giuridicità in San Tommaso d'Aquino*, 2nd edition, Milan, "Vita e Pensiero", 1944, pp. xi + 251, L. 50.

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VIII.—PHILOSOPHICAL PERIODICALS.

REVUE PHILOSOPHIQUE DE LOUVAIN. Tome 44 (3e Série, No. 3), Août 1946. **Suzanne Mansion.** *La première doctrine de la substance : la substance chez Aristote.* [Aristotle was the first philosopher to attach a precise meaning to the term '*οὐσία*' or 'substance'. The doctrine expounded in the *Metaphysics*, the *Posterior Analytics*, and the *Physics* is that substance has four formal properties. (*a*) It is *τὸ ὑποκείμενον*—that which is the subject of predication, but is not itself predicated of anything. (*b*) It is *χωριστόν*—that which is capable of separate existence. (*c*) It is *τόδε τι*—a determinate something. (*d*) It is *τὸ τί ἦν εἶναι*—a thing possessed of essential attributes, in contrast with its accidental attributes, as 'humanity' in Socrates is in contrast with 'whiteness'. Does this doctrine agree with that expounded in the *Categories*, a work that recent investigators regard as of doubtful authenticity? Two differences can be noted. Firstly, the doctrine of the ultimate kinds of being is not expounded in relation to the act of judgment, as in the other works; they are spoken of as 'unrelated expressions.' Secondly, two degrees of substance are distinguished—first substances, like Socrates, that are not in anything else, and are not predicated of anything, and second substances, like man, that are not in anything else, but are predicated of other things. The fundamental attribute of substance is therefore no longer, *τὸ μὴ καθ' ὑποκειμένου λέγεσθαι*, but *τὸ μὴ ἐν ὑποκειμένῳ εἶναι*. You cannot subsume anything under *this most*

specific red, any more than you can subsume anything under *Socrates*. This is a very remarkable change of emphasis. Is it due to the fact that the *Categories* was written by Aristotle before he had fully worked out his metaphysical doctrines, or to the fact that it was written by a young disciple anxious to distinguish two different usages of the phrase τὸ μὴ καθ' ὑποκειμένου λέγεσθαι ? The latter hypothesis is thought the more probable, because the distinction between first and second substances is an advance in logical articulation, but betrays a confused understanding of Aristotle's fundamental doctrine of substance. It would be interesting to hear Jaeger's opinion on this ingenious deduction.] **Robert Feys.** *Les méthodes récentes de déduction naturelle.* [An account of Gentzen's attempt to construct formal deductive systems that approximate to those found in traditional logic and ordinary discourse. The operators used are *All, any, some, not, and, or, implies.* Two groups of calculi are distinguished. N-calculi, which use *assertions with suppositions*, and L-calculi, which use *assertions of consequence.* (1) *N-Calculi.* The fundamental principle is that a premiss implies what can be deduced from it. Feys uses '$:: p$' to mean 'the assertion of p', '$1 :: p$' to mean 'the assertion of p as a first supposition', $\frac{`2 :: p'}{q}$ to mean 'q is deducible from the assertion of p as a second supposition'. The fundamental schemas are those of Conjunction, Alternation, Implication, Negation, Absurdity, Attenuation, Permutation, Contraction, Universalisation and Particularisation. Thus the first rule of implication

$$\frac{\begin{array}{c} n :: \mathrm{P} \\ n :: \mathrm{Q} \end{array}}{:: \mathrm{P\ F\ Q},}$$

which may be translated 'The assertion that P implies Q is deducible from the fact that the assertion of Q is deducible from the assertion of P as an nth supposition'. The resulting system is the intuitionist calculus (NJ), which will not validate the whole of traditional deductive logic (NK), because it does not demonstrate the law of the excluded middle. Two methods of transforming N J into N K are mentioned, those of Gentzen and Bernays. Johansson, by denying the rule 'ex falso sequitur quodlibet', reduces NJ to a Minimal Calculus, which is designated NM. (2) *L-Calculi.* These may be regarded as a particularly explicit form of the calculus of propositions with suppositions. The propositions punctuated with commas before the sign of assertion '::' are to be regarded as linked by conjunction (&); the propositions punctuated with commas after it are to be regarded as linked by alternation (V). Thus $\frac{`p :: p \quad \mathrm{N}p :: \mathrm{N}p'}{\mathrm{N}p, \quad p ::}$ is the formal counterpart of the statement 'It may be deduced that the assertion of any proposition follows from the conjunction of not-p and p, when the assertion of p follows from the supposition of p, and the assertion of not-p follows from the supposition of not-p'. The fundamental schemas of this system are on the same lines as those of NJ. Two interesting peculiarities are the admission of a schema for eliminating middle terms which form part of the consequent of one premiss, and part of the antecedent of another, and the fact that the deductions are constructive, not substitutional, which means, apparently, that once a term enters the deduction it is not dropped, unless it is a doublet, one of whose occurrences can be treated as redundant. Feys exposition is, as usual, remarkably lucid.] **Franz Grégoire.** *Note sur les termes "intuition" et "expérience."* ['Intuition,

in the strictest sense, means the immediate apprehension of individual existence. 'Experience', in the strictest sense, means immediate apprehension of individual existence through sensible awareness, including under this heading all the sensuous elements in consciousness. Other extended, but less proper, uses of these terms are enumerated.]

ÉTUDES CRITIQUES. **André Maes.** *La notion de verité chez W. James.* [Shows that James did not abandon his voluntarist definition of truth under the influence of criticism. To the end he maintained that true beliefs can be distinguished from false ones only by the success or failure of the practical attitudes of which they form part.] Chronique.

IX.—NOTES.

Department of Philosophy,
Bryn Mawr College,
Bryn Mawr, Pa.,
16th October, 1946.

To the Editor of MIND,

SIR,

Mr. E. F. Carritt's review of my book, *Aesthetic Experience and Its Presuppositions*, puts forward a number of opinions and evaluations. As regards these, I was pleased to learn that Mr. Carritt discovered some merit in the book and I was pleased, as well, to be offered some illuminating criticism. I am concerned in this letter, however, with but one of these opinions and with the facts upon which it is based. Mr. Carritt concludes his review with the opinion in question, remarking that the "cumulative effects" of lack of clarification and defects of style "come near to being disastrous" to the book. He writes that the "most obscure" sentences are "too long for quotation in a review" but he does present four quotations which, in his opinion, "exemplify" these defects and, in my opinion, raise the simple questions of fact with which I propose to deal:

(1) Mr. Carritt remarks that "without doubt an *i* in the note on page 261 would be less shocking as an *e*". Mr. Carritt's remark should have been directed to Dr. Samuel Johnson. The footnote in question is a direct quotation (given within quotation marks) from Johnson's *The Rambler* (Oxford edition, Vol. 2, p. 446). It may be of interest that Johnson has some manuscript authority for the reading he gives. In any case, however, my responsibility ends with accurate quotation.

(2) Mr. Carritt writes, "I presume that a 'footless errand' should be a 'bootless' one". I should not accept the emendation. The errand in question is not without reward. It is without footing, basis, or foundation. Mr. Carritt may consult either *A New English Dictionary* or the *Century Dictionary* on this usage, or refer to Coleridge's *Eolian Harp*, which is cited in the former.

(3) Mr. Carritt writes that "On page 300 we are told both that Zeller does not believe 'that *catharsis* is the purification of the soul from a feeling' and that 'in his view . . . the feelings aroused by art are purified'". Mr. Carritt concedes that "no doubt these two sentences could be interpreted in non-contradictory senses, but clarification is desirable". I believe that there is a clear distinction between purifying the soul and purifying the feelings, as well as an evident difference in the meanings of "purified"

as "being purged" and "being pure". Were clarification needed, however, of Zeller's intention, it is supplied on the same page 300 in an additional quotation: Art, writes Zeller, "delivers us from such (emotions) as are morbid or oppressive by exciting such as are subordinate to its laws. . . ."

(4) Mr. Carritt maintains that he cannot construe the following sentence: "The problem remains, therefore, whether on these grounds the assumption that the serious non-æsthetic theory of the origin and nature of art is competent fully to account for each and every product of craft". I should certainly not maintain that the sentence is a contribution to great English prose. I do maintain, however, that it is grammatical and can be construed.

That there are errors and defects in my book, I should not doubt. That their cumulative effect is, as Mr. Carritt maintains, "near to being disastrous", I take leave to doubt in view of the instances he cites. Some of the errors in the book are probably there for the same reason that there occur in Mr. Carritt's review one mis-spelling and two inaccuracies of quotation: printers, not authors, have the last opportunity to err, once final proof-sheets have been examined. In any case, I may assure Mr. Carritt that the errors are not due to carelessness on my part. I may be permitted to doubt, also, that they are due to the "unfamiliarity" of my style. The reviewer in the *Times Literary Supplement* of July 6 appeared to encounter no such difficulties. And, as I recall, my tutors at Hertford College appeared to understand what I wrote as a student there. I doubt that Hertford and University Colleges are in fact farther apart linguistically than they are geographically.

Yours sincerely,
MILTON C. NAHM.

GRADUATE FELLOWSHIPS IN THE UNIVERSITY OF NORTH CAROLINA

The University of North Carolina offers for the academic year 1947/48 a number of graduate fellowships in philosophy, approximately seven (Kenan Fellowships and Williams Fellowships). The stipend is $750.00. Applications should be addressed to: The Graduate School, University of North Carolina, Chapel Hill, N. C.

DEATH OF MR. H. STURT

We regret to announce the death, on December 13th, of our Hon. Treasurer, Mr. H. Sturt. An Obituary Notice will appear in a future issue. Until further notice all correspondence intended for the Treasurer should be addressed to the Hon. Secretary, Mr. H. L. A. Hart, New College, Oxford.

MIND ASSOCIATION.

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